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ABSTRACT

The effectiveness of the Saturation Work Initiative Model (SWIM) was evaluated after its fifth year of operation in San Diego. Data on SWIM on participants' employment, earnings, and Aid to Families with Dependent Children (AFDC) receipt were compared to those persons in one of several control groups consisting of members of the AFDC-unemployed parent and AFDC-family group categories. The sizes of the experimental and control samples studied varied depending on duration of participation in the program; however, the sizes of the experimental and control samples considered in any given analysis were always nearly identical to one another. SWIM proved to be a highly successful and cost-effective program. It increased participants' earnings over the 5-year follow-up period by $2,076 annually per single-parent family and $1,060 annually per two-parent family. SWIM participation accelerated job finding and resulted in AFDC reductions of almost $2,000 per experimental single- and two-parent family over the 5-year period. With net costs of about $900 per participant, SWIM produced substantial gains for government budgets. (Appended are results of Greater Avenues for Independence [GAIN] casefile reviews, supplemental tables, and a subgroup benefit-cost analysis. Forty-one tables and 19 references are included.) (MN)

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The Saturation Work Initiative Model in San Diego:

A Five-Year Follow-up Study

Daniel Friedlander
Gayle Hamilton

July 1993

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THE SATURATION WORK INITIATIVE MODEL IN SAN DIEGO:

A FIVE-YEAR FOLLOW-UP STUDY

Daniel Friedlander
Gayle Hamilton

Manpower Demonstration
Research Corporation

July 1993

The SWIM program was one of two sites included in the federal Demonstration of Saturation Work Programs in an Urban Area. The original two-year follow-up study of SWIM was conducted under a contract with the California State Department of Social Services, with support from the U.S. Department of Health and Human Services (HHS). This five-year study of SWIM was funded by HHS under a competitive award, Contract No. HHS-100-89-0030, to evaluate the Job Opportunities and Basic Skills Training (JOBS) Program of the Family Support Act of 1988. Reproduction by the United States Government in whole or in part is permitted for any purpose.

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The Authors

PREFACE

This report presents five-year results on the effectiveness of the Saturation Work Initiative Model (SWIM) in San Diego, a program that occupies an important place in the evolution of welfare-to-work programs. The results are opportune, coming at a time when welfare reform is an important topic on the nation's domestic policy agenda and findings on the long-term results of welfare-to-work programs are scarce.

SWIM was operated by the County of San Diego, California, from 1985 to 1987 as part of a demonstration sponsored by the U.S. Department of Health and Human Services. Targeted to those applying for or receiving benefits under the Aid to Families with Dependent Children (AFDC) Program, SWIM sought to maximize participation in employment-promoting activities among heads of single-parent families without preschool-age children (mostly women) and heads of two-parent families (mostly men). Individuals who did not participate could be sanctioned with a partial, temporary AFDC grant reduction.

Like many programs of the 1980s, SWIM provided job search and unpaid work experience – activities intended to quickly transition individuals into the work force. But SWIM also included education and training activities, which were assigned after individuals completed job search and unpaid work experience without finding regular employment. Education and training represented investments in people, which were expected to pay off in the longer run. In its inclusion of these services, its requirement that individuals participate on an ongoing basis as long as they remained on welfare, and its monthly participation goals, SWIM anticipated some of the features emphasized in the Job Opportunities and Basic Skills Training (JOBS) Program of the Family Support Act of 1988.

The report's results are based on a comparison of individuals who were randomly assigned to either an experimental group (whose members were required to participate in SWIM) or a control group (whose members were not eligible for SWIM but could, on their own initiative, enroll in community education and training programs). For reasons discussed in the report, the results – which are both encouraging and cautionary – are probably a conservative estimate of the long-term effectiveness of programs such as SWIM. The key findings include:

- *SWIM increased earnings over the five-year follow-up period.* The program produced cumulative five-year earnings gains that averaged $2,076 per single-parent-family experimental (a 14.8 percent increase over the average for controls) and $1,060 per two-parent-family experimental (a 4.9 percent increase over controls). For both single parents and heads of two-parent families, the experimental-control earnings difference narrowed considerably by the end of the five-year follow-up period.

- *SWIM accelerated job-finding.* Experimentals who eventually would have found a job during the five years obtained employment faster because of the program. In addition, SWIM led to employment for some individuals who otherwise would never have worked at all during the five years. However, the pay rates of jobs were similar among the experimentals and controls who did work during the five years.

- *SWIM resulted in AFDC reductions.* Five-year reductions in AFDC payments totaled almost $2,000 per experimental for single-parent and two-parent families – savings that were more than twice the program's net costs. These savings were achieved primarily by SWIM's hastening the departure from the welfare rolls of people who would have left AFDC by the end of the five-year follow-up period, i.e., SWIM had little effect on the proportion of individuals on welfare at the end of the follow-up period.

- *With net costs of about $900 per experimental, SWIM produced substantial gains for government budgets.* For every dollar spent, SWIM returned more than $2.30 per single-parent-family experimental and more than $2.40 per two-parent-family experimental. SWIM did not, however, increase family income, at least as measured in this study: Gains in earnings were usually completely offset by reductions in welfare income.

- *SWIM produced earnings gains and AFDC reductions for a variety of subgroups.* In addition, the results indicated that although net program costs were higher for the more disadvantaged subgroups, even they produced savings for government budgets that fully covered the net costs of including them in SWIM. Considered in conjunction with past research, these findings support working with a broad spectrum of individuals eligible for welfare-to-work programs, from the most to the least "job-ready."

SWIM was thus successful in meeting several fundamental goals sought by welfare-to-work programs and welfare reform policies: It conditioned receipt of full AFDC benefits on continuing participation in the program; moved people into jobs and off AFDC sooner than would otherwise have been the case; and, as a consequence, resulted in substantial savings of government funds. SWIM was less successful in achieving goals often stated for other programs but not explicitly sought in SWIM: It did not lead to better jobs; did not increase measurable total family income; and failed to substantially reduce the number of individuals who were jobless and receiving AFDC at the end of five years. The results thus suggest that the greatest challenge for future welfare-to-work programs will be to find program approaches that can more effectively increase the employment rate and earnings of the most disadvantaged AFDC recipients.

Judith M. Gueron
President

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EXECUTIVE SUMMARY

From July 1985 through September 1987, the County of San Diego, California, operated the Saturation Work Initiative Model (SWIM) as part of a demonstration sponsored by the U.S. Department of Health and Human Services (HHS). The demonstration tested the feasibility and effectiveness of requiring ongoing participation in employment-directed activities by a high proportion of the then-mandatory welfare caseload. Targeted to those applying for or receiving benefits under the Aid to Families with Dependent Children (AFDC) Program, SWIM sought to maximize individuals' participation in job search activities, unpaid community work experience, and education or job training courses. Individuals who did not participate could be sanctioned with a partial, temporary AFDC grant reduction. SWIM was mandatory for case heads, mostly female, of single-parent families (known in California as AFDC-Family Group or AFDC-FGs) without preschool-age children and for all case heads, mostly male, of two-parent (AFDC-Unemployed Parent or AFDC-U) families.

In its establishment of monthly participation targets and its inclusion of education and training activities, SWIM anticipated features of the national Job Opportunities and Basic Skills Training (JOBS) Program established by the Family Support Act (FSA) of 1988, which is currently operating in all states. But JOBS, in combination with other provisions of FSA, differs from SWIM in key ways: It extends a participation mandate to single parents with children as young as age three, or age one at state option (SWIM required participation among those with children age six or older); offers funding for schooling or training (SWIM referred enrollees to providers in the community); gives states flexibility in how to sequence program services (SWIM consisted of a fixed sequence of activities, with program enrollees starting off in job search activities followed by three months of community work experience and then education or training); offers financial incentives to serve long-term AFDC recipients; and provides medical assistance and child care benefits for up to 12 months to people who leave AFDC because they get a job.

The Manpower Demonstration Research Corporation (MDRC) conducted a two-year follow-up study of SWIM under a contract with the California State Department of Social Services, with support from the U.S. Department of Health and Human Services (HHS). This report presents the results of a five-year study of SWIM, which was funded by HHS as part of its JOBS evaluation, and builds on the prior, two-year follow-up findings, which were published in 1989.[1] In brief, these earlier results indicated that SWIM succeeded in imposing a continuous, ongoing participation requirement on a substantial proportion of the then-mandatory welfare population: In a typical month, about 50 percent of those enrolled in the program participated in it at least to some extent (with those who were employed part time or already in a SWIM-approved education or training program counted as participating). SWIM also achieved substantial impacts on earnings and welfare payments, which in many cases exceeded those measured for welfare employment programs operated prior to SWIM. In general, these impacts were found for both AFDC applicants and recipients, and for single parents and heads of two-parent households. Finally, at the two-year conclusion of program operations, about 11 percent of AFDC-FG "experimentals" (see below) had, at some point, been sanctioned (i.e., their monthly AFDC grant had been temporarily reduced because of noncompliance with program rules).

This report presents estimates of SWIM's impacts on employment, earnings, and AFDC receipt over a five-year follow-up period – unusually long for evaluations of welfare-to-work programs. It thus provides important information about the longer-term effects of this approach to building a JOBS program. The report also analyzes SWIM's benefits and costs, from the perspectives of both SWIM enrollees and government budgets. In addition, participation, impact, and benefit-cost results are presented for several subgroups of SWIM enrollees, e.g., short- and long-term AFDC recipients.[2]

The results of the SWIM evaluation are based on an experimental design: Program enrollees were randomly assigned to either an experimental group ("experimentals"), whose members were required to participate in SWIM, or a control group ("controls"), whose members were not eligible for SWIM activities but could, on their own initiative, enroll in community education and training programs. Since controls and experimentals differed only in their eligibility for SWIM, any variations in the two groups' employment, earnings, and welfare receipt can be confidently attributed to SWIM, and thus represent estimates of its impacts. The impacts – often expressed in phrases such as "SWIM increased earnings," "SWIM reduced welfare," or "SWIM led to employment for some who otherwise would never have worked" – refer to the observed behavior of the experimentals relative to the behavior of the controls. Finally, it is the impact estimates, compared to program costs, that yield the cost-effectiveness findings for the program.

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[1] See Gayle Hamilton and Daniel Friedlander, *Final Report on the Saturation Work Initiative Model in San Diego* (New York: MDRC, 1989).

[2] For further analysis of participation in SWIM, see Gayle Hamilton and Daniel Friedlander, *Participation in Welfare-to-Work Programs: Lessons from the Saturation Work Initiative Model in San Diego* (New York: MDRC, forthcoming). Additional results for SWIM also may be found in a forthcoming study by Daniel Friedlander and Gary Burtless, which examines employment stability and AFDC recidivism over five years in four welfare-to-work programs. SWIM results can be compared most readily with those for other broad-coverage, experimentally evaluated programs. See Judith M. Gueron and Edward Pauly, *From Welfare to Work* (New York: Russell Sage Foundation, 1991); Daniel Friedlander and Judith M. Gueron, "Are High-Cost Services More Effective than Low-Cost Services?" in Charles F. Manski and Irwin Garfinkel, eds., *Evaluating Welfare and Training Programs* (Cambridge: Harvard University Press, 1992); and James Riccio and Daniel Friedlander, *GAIN: Program Strategies, Participation Patterns, and First-Year Impacts in Six Counties* (New York: MDRC, 1992). See also David Greenberg and Michael Wiseman, "What Did the OBRA Demonstrations Do?" in Manski and Garfinkel, *Evaluating Welfare and Training Programs*.

An Overview of the Findings

SWIM produced cumulative five-year earnings gains that averaged $2,076 per AFDC-FG experimental (a 14.8 percent increase over the average for controls) and $1,060 per AFDC-U experimental (a 4.9 percent increase). For the AFDC-FG (single-parent) group, this is more than double the two-year earnings impact. For them, the earnings impact reached its peak in follow-up year two, when experimentals earned a statistically significant $644 more than controls, on average. For AFDC-Us, earnings impacts peaked in years one and two, at approximately $480 in each of those years. For both groups, the experimental-control earnings differences narrowed considerably in subsequent follow-up years, reaching $148 for AFDC-FGs and minus $251 for AFDC-Us by year five (neither impact was statistically significant).

SWIM's services and mandates appear to have accelerated job-finding, i.e., experimentals who eventually would have found a job during the five years obtained employment faster because of SWIM. However, average earnings were quite similar for employed experimentals and employed controls. SWIM also led to employment, at some point during the follow-up period, for about one out of every five experimentals who otherwise would never have worked during this period.

SWIM resulted in five-year reductions in AFDC payments of almost $2,000 per experimental for single-parent and two-parent families, savings that were more than twice the program's net costs. These reductions represented a 10.9 and 9.3 percent decrease, respectively, relative to the AFDC-FG and AFDC-U control group averages. For both AFDC-FGs and AFDC-Us, impacts on AFDC payments peaked in follow-up year two (averaging about $560 in that year), were slightly lower in year three, and then began to decline more sharply but remained statistically significant for AFDC-FGs through year four.

For AFDC-FGs, the great bulk of AFDC savings came from their receiving welfare for fewer months rather than from lower monthly payments. This suggests that sanctions, which reduce but do not eliminate the monthly grant for AFDC-FGs, probably made no more than a small direct contribution to AFDC savings. However, the threat of sanctions may have indirectly affected the impacts, e.g., if the threat of a sanction prompted an experimental to participate in a program activity. The results also indicate that SWIM hastened the departure from the rolls of people who would have left AFDC in any case. However, SWIM had only a modest effect on the proportion of enrollees on welfare at the end of the five-year follow-up period.

With net costs of about $900 per experimental, SWIM produced substantial gains for government budgets: For every dollar spent, SWIM returned more than $2.30 per AFDC-FG experimental and more than $2.40 per AFDC-U experimental. On average, however, SWIM did not improve the financial position of individuals subject to the program: Gains in experimentals' earnings were usually completely offset by reductions in their welfare income. In addition, it should be noted that following a group of SWIM enrollees for five years appears to have captured most of the program's impacts; further effects beyond the five-year point are likely to be small.

Earnings gains and AFDC reductions were found for a variety of subgroups in SWIM. Overall, earnings impacts tended to be larger for AFDC-FG and AFDC-U subgroups with fewer barriers to employment, e.g., individuals who had a high school diploma or GED (high school equivalency credential) or those with higher earnings in the year prior to enrolling in SWIM. Welfare impacts were distributed more evenly than earnings impacts across AFDC-FG subgroups, but were largest for the most disadvantaged AFDC-U subgroup, i.e., recipients with more than two years on their own AFDC case, with no earnings in the previous year, and without a high school diploma or its equivalent. The subgroup benefit-cost analysis indicated that, although net program costs were higher for the more disadvantaged subgroups, even they produced savings for government budgets that fully covered the net costs of including them in SWIM. Considered in conjunction with past research, these findings support working with a broad spectrum of individuals eligible for welfare-to-work programs, from the most to the least "job ready."

Finally, it is likely that the estimated impacts for the latter half of the follow-up period are underestimates of the impact of SWIM had it been a permanent program and not a two-year demonstration. This is because SWIM controls were eligible for, and received, mandatory welfare-to-work program services, through the Greater Avenues for Independence (GAIN) Program, in the third through fifth years of the five-year follow-up period.[3] If controls had not received these services, it is likely that they would have "caught up" to experimentals more slowly, resulting in a larger and longer-lasting experimental-control difference in earnings and AFDC receipt in the latter half of the follow-up period.

Assessing SWIM's Long-Term Results

In assessing the five-year SWIM results, it is important to keep in mind two key considerations: SWIM's goals and the nature of welfare dynamics. Welfare-to-work programs have a variety of possible goals, which are sometimes conflicting and which are given more or less weight by different policymakers. Possible goals include: imposing a participation requirement on those targeted for the program; increasing overall employment and earnings levels among AFDC recipients; reducing the overall level of AFDC receipt; reducing the level of AFDC receipt among long-term or potential long-term AFDC recipients; increasing individuals' self-sufficiency (e.g., increasing the proportion of income that individuals receive from earnings and decreasing the proportion obtained from AFDC); saving money for government budgets by reducing AFDC and other welfare expenditures; increasing the employment of AFDC recipients in high-wage, full-time, stable jobs; making families better off financially, regardless of the sources of their income; and reducing poverty. Goals emphasized in SWIM included imposing a participation requirement on the entire then-mandatory AFDC caseload, increasing overall employment levels, and reducing AFDC receipt. This report assesses how well SWIM achieved the goals it emphasized and some other goals as well.

To summarize the report's findings: SWIM met the goals of conditioning full AFDC receipt on program participation, increasing employment, reducing AFDC receipt, decreasing AFDC receipt among the most disadvantaged recipients, increasing AFDC recipients' self-sufficiency, and saving money for government budgets. For some of these goals, SWIM's results exceeded those found in previous evaluations. However, SWIM did not lead to "better" jobs for experimentals, make them financially better off, or reduce poverty.

Welfare dynamics are also important to consider in assessing the five-year SWIM results. Extensive research shows that AFDC receipt is a dynamic, not a static, phenomenon. People get married or reconciled or become employed, and their children grow up, making their families ineligible for AFDC. AFDC grants are consequently reduced or terminated even in the absence of a special welfare-to-work program. These dynamics imply that controls will gradually leave AFDC and that eventually all of them will be off the rolls. Even if SWIM moved experimentals off AFDC faster than controls early in the follow-up period, over time the AFDC receipt rates for experimentals and controls must converge at zero. Thus, the impact on AFDC receipt, i.e., the experimental-control difference, must eventually narrow to zero.

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[3] GAIN was SWIM's successor program in San Diego and is California's current, statewide, mandatory JOBS program.

The situation is more complicated for earnings impacts. On the one hand, earnings have no fixed ceiling, and it is possible that welfare-to-work programs could confer on experimentals an initial earnings advantage over controls that would remain permanent. On the other hand, it is possible that as controls begin to find jobs on their own, they will start to "catch up" to experimentals to some degree, resulting in a decrease in the experimental-control earnings differential. This differential may also narrow if the treatment effect on experimentals starts to wear off, which could occur, e.g., if the jobs experimentals initially obtain because of the program do not last very long.

The narrowing of experimental-control differences in employment and AFDC receipt over time is often referred to as impact "decay." Since control catch-up is likely to be even more important than the wearing-off of the program treatment, the more neutral term "convergence" may be more appropriate. Convergence or narrowing does not imply that early impacts are lost. Impacts that accumulate before the experimental-control differential begins to narrow remain real and "bankable," but each year's *additions* to the cumulative impacts of the program will be smaller than the last, and will stop altogether if the experimental-control gap narrows to zero.[4]

Program Model, Context, and Participation Levels

The SWIM program model consisted of a fixed sequence of activities, which could result in individuals getting employed and/or leaving AFDC at any point. Individuals were usually first assigned to a two-week job search workshop. Those who had not found jobs by the time they completed the workshop were assigned to a three-month unpaid work position concurrent with biweekly job club sessions. Unpaid work assignments were set up under the Community Work

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[4] Moreover, convergence of AFDC outcomes within five years of random assignment implies that the program did not have an impact on the number of future long-term AFDC recipients, defined as individuals who were still on AFDC at the five-year mark.

Experience Program (CWEP) and were usually scheduled to be 20 to 30 hours per week. Those still unemployed after completing their work assignment would be assessed and referred to community education and training programs. SWIM did not itself operate these education and training programs but did require that enrollees participate in them after referral.

Two major groups of enrollees were temporarily excused from the aforementioned activities. Individuals with unsubsidized employment of at least 20 hours per week and those who were already engaged in education and training that met program criteria (called "self-initiated participants") were excused from other activities for as long as they remained employed or active in education or training programs. In addition, a small group of enrollees – primarily undocumented workers whose children were U.S. citizens – were deferred from all program participation.

SWIM operated in two of San Diego County's seven welfare employment offices, and served the most urban and disadvantaged part of the county's caseload. In a typical month during the demonstration's second year, about 3,600 individuals were enrolled in SWIM and were eligible for its services.

Because it included education and training, SWIM provided an opportunity to look at the feasibility and effectiveness of mandating such activities. In SWIM, however, these activities were assigned only for those who completed job search and unpaid community work experience without finding a job. In practice, many individuals found jobs or otherwise left AFDC before reaching this third (education and training) stage of activities. SWIM's sequence of activities contrasts with the GAIN program and JOBS programs in some other states, in which basic education is the first assigned activity for those determined to need it. Education may, therefore, play a larger role in producing impacts for those programs than whatever role it played in SWIM.

SWIM was implemented in an inner-city area with several distinguishing features. First, during the period in which SWIM operated, the San Diego labor market was strong. Together with California's relatively high AFDC grant level, this enabled more program enrollees to combine unsubsidized employment with the receipt of AFDC than would be possible in most other states. Part-time employment was allowed to substitute for other SWIM activities, which reduced the number of SWIM enrollees for whom other program activities had to be assigned. Second, San Diego has a comprehensive network of education and training facilities, which both aided SWIM staff in placing experimentals in education and training and increased the likelihood that controls would enroll, on their own initiative, in these activities. Third, the San Diego Department of Social Services had extensive experience successfully implementing welfare-to-work programs, which reduced start-up problems for SWIM and probably resulted in relatively efficient program operations. Finally, San Diego's program funding levels were higher than those for most previous welfare-to-work programs, having been supplemented by special state monies and federal demonstration funds.[5]

Most previous welfare-to-work programs imposed obligations on only a portion of the mandatory caseload, and for only short-term participation. SWIM, in contrast, pursued a "saturation" approach: First, the demonstration explicitly sought to maximize the proportion of the eligible population that participated in the program; second, SWIM required people to participate continuously for as long as they received AFDC. At the beginning of the demonstration, HHS set as a target that, in each month, 75 percent of those enrolled in the program participate in it. Analyses presented in an earlier report[6] indicate that the program achieved close to the maximum rates possible, given its rules and resources: During a typical month, approximately half of all enrollees were active for at least one hour – but usually much more – in job search, unpaid community work experience, education or training (through referrals by SWIM staff or at the individuals' own initiative), or part-time employment. Excluding part-time employment, monthly participation rates averaged 33 percent. To attain these rates, staff worked with almost all enrollees. Most nonparticipants were found to be only temporarily inactive and to have had reasons for not participating that were accepted as legitimate under program rules.

Throughout the course of the demonstration, participation was substantial in all major SWIM activities. A high proportion of experimentals participated in job search and unpaid community work experience, services that were not available to controls. During the first two to three years of the follow-up period (prior to the start of GAIN), more than half of the experimentals – 54 percent of the AFDC-FGs and 60 percent of the AFDC-Us – began a formal job search activity, and approximately one-fifth of the AFDC-FG and AFDC-U experimentals participated in work experience.[7]

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[5] Unpublished MDRC calculations indicate that, in 1986 dollars, the total cost of SWIM, including the worth of services provided and paid for not by SWIM directly but by community education and training agencies, was approximately $14.2 million during its two years of operations. Approximately $7.8 million of this cost was borne by the San Diego Department of Social Services, of which approximately $1.7 million was provided as special demonstration funds by federal and state government agencies.

[6] See Gayle Hamilton, *Interim Report on the Saturation Work Initiative Model in San Diego* (New York: MDRC, 1988).

[7] In the SWIM evaluation, "began an activity" is defined as having participated for at least one day. Federal measures of participation in the JOBS program focus on monthly participation in a variety of activities that average 20 hours per week or more among all participants.

During the pre-GAIN years, SWIM increased enrollment in basic education, training, and college by 10 to 11 percentage points.[8] This is despite the fact that many controls, as well as experimentals, participated in these activities – the controls (and some of the experimentals) having done so on their own initiative. Specifically, more than 39 percent of the AFDC-FG experimentals (vs. 29 percent of the controls) and 34 percent of the AFDC-U experimentals (vs. 23 percent of the controls) began basic education courses (i.e., adult basic education, GED preparation, or English as a Second Language), college-level courses, or vocational training within two to three years following their SWIM enrollment.

The cost of SWIM operations plus the costs to community education and training providers were the "gross costs" of working with the experimental group. From this were subtracted the gross costs of community-provided education and training for controls. The resulting net costs of SWIM were approximately $900 per AFDC-FG experimental and approximately $800 per AFDC-U experimental. Among experimentally evaluated, JOBS-predecessor programs, only one has equalled these levels of resources.

Impact Findings

The impacts of SWIM were estimated by comparing the average earnings and AFDC outcomes for all experimentals to those for all controls.

- **SWIM produced five-year earnings gains that averaged $2,076 per AFDC-FG experimental and $1,060 per AFDC-U experimental. For AFDC-FGs, this was more than double the two-year impact estimated previously.**

Over the five-year follow-up period, total earnings per AFDC-FG control averaged $14,033 compared to $16,109 per AFDC-FG experimental. (See Table ES.1.) This represents a statistically significant gain of $2,076 – a 14.8 percent increase over the average for controls and more than twice the program's net costs. Among AFDC-U controls, total earnings averaged $21,818 over the same five-year period compared to $22,878 for experimentals. (See Table ES.2.) The difference, $1,060, was 4.9 percent above the control group average and exceeded the net program cost, but was not statistically significant. (It is important to note that these impact estimates are averages and include

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[8] Also, AFDC-FG and AFDC-U experimentals remained enrolled, on average, approximately 23 and 24 days longer, respectively, than controls (including in the averages zero days for those who never participated).

TABLE ES.1

SWIM

ALL AFDC-FG: IMPACTS ON EMPLOYMENT, EARNINGS, AFDC RECEIPT, AND AFDC PAYMENTS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Ever employed (%)				
Years 1-5	74.6	67.5	7.1 ***	10.5%
Average quarterly employment rate (%)				
Years 1-5	33.9	29.0	4.8 ***	16.7%
Year 1	33.0	25.7	7.3 ***	28.4%
Year 2	35.2	27.9	7.2 ***	25.9%
Year 3	34.4	28.2	6.2 ***	22.0%
Year 4	33.5	31.3	2.2	7.0%
Year 5	33.3	32.0	1.3	4.0%
Average total earnings ($)				
Years 1-5	16109	14033	2076 **	14.8%
Year 1	2029	1678	352 ***	21.0%
Year 2	2892	2248	644 ***	28.6%
Year 3	3287	2732	555 ***	20.3%
Year 4	3775	3397	378	11.1%
Year 5	4126	3978	148	3.7%
Ever received any AFDC payments (%)				
Years 1-5	92.9	93.2	-0.3	-0.3%
Percent on AFDC at year end (%)				
Year 1	60.9	68.3	-7.4 ***	-10.8%
Year 2	48.3	55.2	-6.9 ***	-12.5%
Year 3	41.3	45.4	-4.1 **	-9.1%
Year 4	36.0	38.4	-2.4	-6.2%
Year 5	31.5	32.5	-1.0	-3.1%
Average total AFDC payments received ($)				
Years 1-5	15726	17642	-1916 ***	-10.9%
Year 1	4419	4838	-419 ***	-8.7%
Year 2	3407	3968	-560 ***	-14.1%
Year 3	2952	3435	-483 ***	-14.1%
Year 4	2621	2905	-284 **	-9.8%
Year 5	2327	2496	-169	-6.8%
Sample size	1604	1606		

SOURCE: MDRC calculations from the State of California Unemployment Insurance quarterly earnings records and County of San Diego monthly AFDC records.

NOTES: These data include zero values for sample members not employed and for sample members not receiving welfare. Rounding may cause slight discrepancies in calculating sums and differences. A two-tailed t-test was applied to differences between experimental and control groups. Statistical significance levels are indicated as: *= 10 percent; **=5 percent; ***=1 percent.

TABLE ES.2

SWIM

ALL AFDC-U: IMPACTS ON EMPLOYMENT, EARNINGS, AFDC RECEIPT, AND AFDC PAYMENTS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Ever employed (%)				
Years 1-5	79.3	75.4	4.0 *	5.2%
Average quarterly employment rate (%)				
Years 1-5	39.4	35.7	3.7 **	10.4%
Year 1	37.5	32.0	5.6 ***	17.5%
Year 2	41.8	36.9	4.8 **	13.1%
Year 3	40.1	36.3	3.8 *	10.4%
Year 4	40.2	37.6	2.6	7.0%
Year 5	37.3	35.6	1.8	4.9%
Average total earnings ($)				
Years 1-5	22878	21818	1060	4.9%
Year 1	3303	2815	487 *	17.3%
Year 2	4308	3831	478	12.5%
Year 3	4797	4448	350	7.9%
Year 4	5211	5214	-4	-0.1%
Year 5	5259	5510	-251	-4.6%
Ever received any AFDC payments (%)				
Years 1-5	90.0	90.2	-0.2	-0.2%
Percent on AFDC at year end (%)				
Year 1	54.6	59.2	-4.6 *	-7.8%
Year 2	47.4	50.3	-2.9	-5.8%
Year 3	41.5	43.5	-2.1	-4.8%
Year 4	40.0	40.3	-0.3	-0.7%
Year 5	40.0	39.7	0.3	0.7%
Average total AFDC payments received ($)				
Years 1-5	19093	21054	-1961 **	-9.3%
Year 1	4888	5303	-415 **	-7.8%
Year 2	3896	4455	-558 ***	-12.5%
Year 3	3558	4036	-479 **	-11.9%
Year 4	3406	3730	-324	-8.7%
Year 5	3345	3530	-185	-5.2%
Sample size	686	654		

SOURCE: MDRC calculations from the State of California Unemployment Insurance quarterly earnings records and County of San Diego monthly AFDC records.

NOTES: These data include zero values for sample members not employed and for sample members not receiving welfare. Rounding may cause slight discrepancies in calculating sums and differences. A two-tailed t-test was applied to differences between experimental and control groups. Statistical significance levels are indicated as: *= 10 percent; **=5 percent; ***=1 percent.

zero earnings for those who did not work; they are not the difference SWIM made in the average earnings of those who worked.)

- **SWIM accelerated job-finding among experimentals, but did not result in higher rates of pay for those who became employed. SWIM also led to employment for some individuals who, in the absence of the program, would not have worked at all during the five-year follow-up period.**

Accelerated job-finding means that enrollees who would have worked anyway found jobs faster because of SWIM. As shown in Tables ES.1 and ES.2, impacts on average quarterly employment rates were highest in the early years of the five-year follow-up period for both AFDC-FGs and AFDC-Us; they then tapered off.[9] Once they were working, however, experimentals and controls had quite similar average earnings (not shown in the tables). The bulk of the impacts on earnings was associated with increases in the amount of time experimentals were employed rather than with higher earnings while they were working.

SWIM also led to employment for some experimentals who would not have worked at all. As shown in Table ES.1, 67.5 percent of the AFDC-FG controls were employed at some time during the five-year follow-up period compared to 74.6 percent of AFDC-FG experimentals – a statistically significant difference of 7.1 percentage points. Considering that 32.5 percent of the controls never worked during the follow-up period, the 7.1 percentage point impact indicates that about one in every five AFDC-FG experimentals who would not have worked in the absence of SWIM did work at some point during the follow-up period. Among AFDC-Us, 75.4 percent of controls were employed during the follow-up period compared to 79.3 percent of the experimentals – a statistically significant difference of 4.0 percentage points.[10] (See Table ES.2.) Again, considering the proportion of AFDC-U controls who never worked during the follow-up period, this impact indicates that about one in six AFDC-U experimentals who would not have worked at all did work as a result of SWIM.

- **For both AFDC-FGs and AFDC-Us, five-year reductions in AFDC payments were large compared to program costs, and the ratio of AFDC reductions to earnings gains was high.**

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[9] Further evidence that SWIM sped up job-finding may be found in the forthcoming study by Daniel Friedlander and Gary Burtless.

[10] Note that, throughout this summary, addition or subtraction may not exactly yield the result indicated owing to rounding.

As shown in Table ES.1, total AFDC payments over the five-year follow-up period averaged $17,642 per AFDC-FG control and $15,726 per experimental. The resulting $1,916 per-experimental reduction was statistically significant and amounted to a 10.9 percent saving relative to the AFDC-FG control group average. As shown in Table ES.2, AFDC payments averaged $21,054 per AFDC-U control and $19,093 per AFDC-U experimental over the same follow-up period. The $1,961 difference was statistically significant and constituted a 9.3 percent reduction relative to the AFDC-U control group average. These reductions were more than twice the net cost of the program for both AFDC-FGs and AFDC-Us. The dollar amount of AFDC impacts was similar to total earnings gains for AFDC-FGs and nearly double the total earnings gains for AFDC-Us.

For AFDC-FGs, the great bulk of AFDC savings came from fewer months of AFDC receipt rather than lower monthly payments (not shown in the tables). In other words, sanctions, which only partially reduce the monthly grant for AFDC-FGs, probably made no more than a small *direct* contribution to AFDC savings. However, the threat of sanctions may (or may not) have contributed indirectly to the program's impact by reinforcing the participation requirement. Lower monthly grants for those remaining on welfare appeared to play a greater role in welfare savings for AFDC-Us than for AFDC-FGs: Less than half the savings for AFDC-Us was associated with fewer months on AFDC. In both assistance categories, a substantial proportion of experimentals – about one-third of the AFDC-FGs and two-fifths of the AFDC-Us – were receiving AFDC payments at the end of the five-year follow-up period. (See Tables ES.1 and ES.2.)

- **For both AFDC-FGs and AFDC-Us, differences between experimentals and controls declined after the first three years of follow-up. Additions to total impacts after year five are likely to be small. This pattern may, in part, reflect controls' exposure to the GAIN program, which started in the third year of the follow-up period.**

For AFDC-FGs, earnings impacts reached their peak in follow-up year two, when experimentals were earning a statistically significant average of $644 more than controls. As shown in Table ES.1, this experimental-control earnings difference narrowed considerably in subsequent follow-up years, reaching $148 (not statistically significant) in year five. For AFDC-Us, earnings impacts reached their peak in years one and two. (See Table ES.2.) In year one, AFDC-U experimentals earned, on average, $487 more than controls, a statistically significant difference. The earnings impacts in year two averaged $478 (not statistically significant). AFDC-U average annual earnings impacts dropped sharply after year three, reaching experimental-control differences of *minus* $4 and $251 (not statistically significant) in years four and five.

For both AFDC-FGs and AFDC-Us, impacts on AFDC payments peaked in follow-up year two, averaging $560 for AFDC-FGs and $558 for AFDC-Us. AFDC impacts for AFDC-FGs and AFDC-Us were slightly below these levels in year three and then began to decline more sharply. However, they remained statistically significant for AFDC-FGs through year four. In year five of the follow-up period, AFDC payments impacts were $169 for AFDC-FGs and $185 for AFDC-Us, neither of which was statistically significant. As indicated earlier, the impact estimates for the last years of follow-up underestimate the effects of a permanent SWIM program because the GAIN program started up (for controls as well as experimentals) during the third year of follow-up. (Control group members' participation in GAIN, a program quite similar to SWIM, was substantial and represented the first exposure to mandatory welfare-to-work programs that controls had had in several years, whereas GAIN was the second "round" of welfare-to-work program exposure for experimentals.)

- **"Convergence" of experimental and control group employment and AFDC receipt over time did not result mainly from "wearing-off" of the treatment effect among experimentals. Rather, it appears to have resulted largely from "catch-up" by controls.**

The narrowing of the experimental-control differential over time is traditionally referred to as impact "decay." This term connotes a process whereby treatment effects, which initially produced impacts, gradually wear off, with individuals eventually leaving their newfound jobs and returning to AFDC. In fact, employment rates for experimentals did peak in year two and decline thereafter, both for AFDC-FGs and AFDC-Us, as shown in Tables ES.1 and ES.2. One possible explanation for this wearing-off of effect is that the SWIM program did not result in experimentals finding "better" jobs, i.e., jobs with higher rates of pay and less likelihood of termination.

More important, however, controls' employment rates increased and their AFDC receipt rates decreased, to the point where controls almost caught up with experimentals by follow-up year four or five. (See Tables ES.1 and ES.2.) Some of this catch-up, particularly in terms of AFDC receipt, resulted from the normal dynamics of welfare receipt. It is also likely, however, that some resulted from controls becoming exposed to San Diego County's GAIN program and receiving its program services in the later years of the five-year follow-up period. This is suggested by findings from the GAIN evaluation, which show that GAIN resulted in earnings gains and AFDC reductions in its first year of follow-up.[11] Because control group catch-up was so important, the more neutral term "convergence" (rather than "decay") seems more appropriate.

Benefit-Cost Findings

- **On average, SWIM did not improve the financial position of individuals subject to the program. For AFDC-FG experimentals, gains in earnings were almost completely offset by losses in welfare income; for AFDC-U experimentals, losses exceeded gains. As a result of SWIM, however, experimentals depended more on employment and less on government transfer programs for their income during the five-year follow-up period.**

The benefit-cost analysis combined information about observed impacts on earnings and AFDC payments and imputed effects on other outcomes. Taking into account earnings gains and fringe benefits associated with employment, AFDC-FG experimentals got total program benefits valued at $2,016 per experimental. Weighing this against increases or decreases in taxes, foregone AFDC, and changes in other government transfers yielded a net gain of only $126 per AFDC-FG experimental over the five-year follow-up period. Thus, AFDC-FGs subject to SWIM broken even, approximately. AFDC-U experimentals, on average, got total program benefits valued at $1,260 and incurred a net loss of $593 over the five-year period. This loss reflects the fact that AFDC-U earnings impacts dropped more sharply during the follow-up period than did those of AFDC-FGs.

- **From the benefit-cost perspective of government budgets (federal, state, and local), SWIM produced substantial gains: Savings connected with AFDC grant reductions greatly exceeded SWIM's net costs. For every dollar spent by government, SWIM returned at least $2.30 per AFDC-FG experimental and at least $2.40 per AFDC-U experimental.**

During the five-year follow-up period, reductions in AFDC and other government expenditures totaled $2,153 per AFDC-FG experimental, whereas the net cost of operating SWIM over the two-year period (the experimental-control difference in dollar investments in the program) was $920. From the perspective of government budgets, this represented savings, per AFDC-FG experimental, of $1,234. Results for AFDC-Us were similar. Reductions in government expenditures totaled $2,028 per AFDC-U experimental, and the net two-year cost of SWIM per AFDC-U experimental totalled $840, yielding savings to government of $1,188 per AFDC-U experimental. These benefit-cost results might have been different had SWIM been a permanent program, i.e., if, in the later follow-up years, experimentals had been subject to SWIM, and not GAIN, and controls had not been exposed to GAIN or subject to its participation mandate.

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[11] See Riccio and Friedlander, 1992.

Subgroup Findings

To inform program targeting policies, the report examines SWIM's impacts on subgroups of the total sample, which were defined using characteristics data collected just prior to random assignment. Selected results are shown in Tables ES.3 and ES.4. The report also draws comparisons between the SWIM subgroup results and those presented in an earlier analysis of subgroup impacts for AFDC-FGs in five low- to moderate-cost, broad-coverage, JOBS-predecessor welfare-to-work programs of the 1980s.[12]

The first set of subgroups, shown in Tables ES.3 and ES.4 under the category labeled "barriers to employment," reflects selected characteristics associated with earning power. Individuals likely to have had fewer barriers to employment were those who had a high school diploma (or its equivalent) or relatively high prior-year earnings. The second set of subgroups, shown under the category labeled "AFDC history and status," divided the full sample up according to several criteria: length of time receiving AFDC, welfare status (i.e., whether an individual was applying for AFDC or already receiving AFDC as of the time she or he became part of the study), and subgroups based on a combination of characteristics associated with "disadvantagedness." This "level of disadvantage" category divided the sample into four groups: applicants who had never had their own AFDC case; applicants returning to AFDC after a "spell" of not receiving AFDC; recipients, labeled "more disadvantaged," who had their own AFDC case for more than two years, had no earnings in the previous year, and lacked a high school diploma or its equivalent; and the rest of the recipients, labeled "less disadvantaged," who did not meet those criteria. The third and final subgroup set divided the sample according to ethnicity.

- **Earnings gains and AFDC reductions were found for a variety of subgroups. Although differences in impacts across subgroups were usually not statistically significant, earnings impacts tended to be larger for AFDC-FG and AFDC-U subgroups with fewer barriers to employment and AFDC-FGs with shorter**

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[12] See Daniel Friedlander, *Subgroup Impacts and Performance Indicators for Selected Welfare Employment Programs* (New York: MDRC, 1988).

TABLE ES.3

SWIM

AFDC-FG SUBGROUPS: IMPACTS ON EARNINGS AND AFDC PAYMENTS

		Average Total Earnings Years 1-5 ($)				Average Total AFDC Payments Years 1-5 ($)			
Subgroup	Percent of Sample	Experi-mentals	Controls	Difference	(a)	Experi-mentals	Controls	Difference	(a)
FULL SAMPLE	100.0	16109	14033	2076	**	15726	17642	-1916	***
BARRIERS TO EMPLOYMENT									
Prior year earnings									
$3,000 or more	17.7	31079	27267	3812	*	11641	12440	-800	
$1-$2,999	21.6	16746	15858	887		14982	16614	-1632	*
None	60.7	11518	9537	1982	*	17198	19531	-2332	***
High school diploma									
Yes	56.1	20540	18135	2405	**	13904	15945	-2040	***
No	43.9	10437	8783	1654		18059	19815	-1756	***
AFDC HISTORY AND STATUS									
Had own AFDC case									
Never	11.2	21030	15806	5225	**	11128	12497	-1369	
Two years or less	20.2	21659	19108	2551		10698	12429	-1731	*
More than two years	68.6	13651	12237	1414		17949	20020	-2072	***
Welfare status									
Applicant	39.2	17911	17030	881		11206	13046	-1841	***
Recipient	60.8	14968	12122	2845	***	18635	20599	-1964	***
Level of disadvantage				x					
First-time applicants	8.6	22483	16285	6198	**	9002	11215	-2213	
Returning applicants	30.6	16620	17242	-622		11811	13577	-1767	**
Less disadvantaged recipients	42.4	18247	14833	3415	***	17484	19499	-2015	***
More disadvantaged recipients	18.4	7332	5850	1483		21218	23201	-1983	**

(continued)

Table ES.3 (continued).

Subgroup	Percent of Sample	Average Total Earnings Years 1-5 ($)			Average Total AFDC Payments Years 1-5 ($)		
		Experimentals	Controls	Difference (a)	Experimentals	Controls	Difference (a)
ETHNICITY							
White, non-Hispanic	27.3	17963	16546	1417	11803	14134	-2331 ***
Black, non-Hispanic	42.4	14846	13926	920	17250	18856	-1606 **
Hispanic	25.4	15140	11402	3739 **	17435	19570	-2135 **
Asian and other	4.9	22707	14612	8095 **	16000	16631	-632
Sample size	(total= 3210)	1604	1606		1604	1606	

SOURCE: MDRC calculations from the State of California Unemployment Insurance quarterly earnings records and County of San Diego monthly AFDC records. Demographic and other background data, used to define subgroups, were obtained from a one-page questionnaire administered to sample members just prior to random assignment.

NOTES: These data include zero values for sample members not employed and for sample members not receiving welfare. Rounding may cause slight discrepancies in calculating sums and differences. A two-tailed t-test was applied to differences between experimental and control groups. Statistical significance levels are indicated as: *=10 percent; **=5 percent; ***=1 percent.

(a) An F-test was applied to subgroup differences in "difference" estimates. Statistical significance levels are indicated as: x = 10 percent; xx=5 percent; xxx = 1 percent.

TABLE ES.4

SWIM

AFDC-U SUBGROUPS: IMPACTS ON EARNINGS AND AFDC PAYMENTS

Subgroup	Percent of Sample	Average Total Earnings Years 1-5 ($)			Average Total AFDC Payments Years 1-5 ($)		
		Experi-mentals	Controls	Difference (a)	Experi-mentals	Controls	Difference (a)
FULL SAMPLE	100.0	22878	21818	1060	19093	21054	-1961 **
BARRIERS TO EMPLOYMENT							
Prior year earnings							
$3,000 or more	32.1	37973	35689	2284	15573	15933	-359
$1-$2,999	24.5	19407	19242	165	18429	21819	-3391 **
None	43.4	13707	12988	719	22072	24393	-2321 *
High school diploma							x
Yes	47.0	27686	24645	3041	17478	17710	-232
No	53.0	18651	19329	-678	20559	24038	-3478 ***
AFDC HISTORY AND STATUS							
Had own AFDC case							
Never	33.7	28125	27152	972	14767	14294	473
Two years or less	34.6	23088	23301	-213	17596	20447	-2851 **
More than two years	31.8	17087	14600	2487	25242	28814	-3572 **
Welfare status							x
Applicant	59.5	27573	25574	1999	15512	16305	-793
Recipient	40.5	16034	16342	-308	24407	28070	-3663 ***
Level of disadvantage							xx
First-time applicants	30.6	29163	28016	1146	13842	13050	792
Returning applicants	28.9	25861	23124	2737	17190	19625	-2435
Less disadvantaged recipients	29.3	17740	19413	-1673	23567	25905	-2337
More disadvantaged recipients	11.2	11795	8436	3360	26313	33681	-7369 ***

(continued)

Table ES.4 (continued).

Subgroup	Percent of Sample	Average Total Earnings Years 1-5 ($) Experi-mentals	Controls	Difference (a)	Average Total AFDC Payments Years 1-5 ($) Experi-mentals	Controls	Difference (a)
ETHNICITY							xx
White, non-Hispanic	25.6	22729	22978	-249	13176	14162	-986
Black, non-Hispanic	20.8	23433	20276	3158	22382	22349	33
Hispanic	40.2	25873	22703	3169	19886	21013	-1127
Asian and other	13.4	13157	19205	-6048	22954	31962	-9008 *
Sample size	(total= 1340)	686	654		686	654	

SOURCE: MDRC calculations from the State of California Unemployment Insurance quarterly earnings records and County of San Diego monthly AFDC records. Demographic and other background data, used to define subgroups, were obtained from a one-page questionnaire administered to sample members just prior to random assignment.

NOTES: These data include zero values for sample members not employed and for sample members not receiving welfare. Rounding may cause slight discrepancies in calculating sums and differences. A two-tailed t-test was applied to differences between experimental and control groups. Statistical significance levels are indicated as: *=10 percent; **=5 percent; ***=1 percent.

(a) An F-test was applied to subgroup differences in "difference" estimates. Statistical significance levels are indicated as: x = 10 percent; xx = 5 percent; xxx = 1 percent.

welfare histories. Among AFDC-FGs, welfare impacts were distributed more evenly across subgroups; among AFDC-Us, welfare impacts were larger for the more disadvantaged subgroups.

As shown in Table ES.3, statistically significant earnings and AFDC impacts were found for several AFDC-FG subgroups. There is some indication that earnings impacts were generally greater for subgroups facing fewer barriers to employment and for those with shorter AFDC histories. However, the *difference* in impacts across subgroups was usually not statistically significant. In addition, this pattern was not uniform. For example, less disadvantaged AFDC recipients had larger earnings impacts than did applicants returning to the AFDC rolls (a presumably more job-ready group).

Statistically significant AFDC impacts were observed for most AFDC-FG subgroups. AFDC impacts were distributed fairly evenly across some subgroup dimensions, although subgroups based on prior earnings and the length of time the individual had her own AFDC case showed AFDC impacts increasing from the less to the more disadvantaged. The *differences* in impacts across subgroups were not statistically significant, however.

The patterns of impacts on earnings and AFDC also differed among AFDC-U subgroups. As shown in Table ES.4, while many subgroups experienced earnings impacts, these impacts tended to be larger for subgroups facing fewer employment barriers. For example, earnings impacts were greater for individuals with higher earnings in the year prior to SWIM enrollment. However, earnings impacts were greater for individuals with longer AFDC histories. Note that none of the AFDC-U subgroup impacts on earnings were statistically significant, and none of the *differences* in earnings impacts across subgroups were statistically significant, either.

AFDC-U subgroup differences in impacts on AFDC payments were more likely to be statistically significant and exhibited a clearer pattern. Statistically significant impacts were found only among those with greater barriers to employment and those considered to be more disadvantaged, i.e., individuals with scant prior-year earnings or none, those lacking a high school diploma or GED, enrollees with at least some previous experience receiving AFDC, and AFDC recipients considered to be more disadvantaged. It is notable that several of the differences across subgroups were statistically significant.

Subgroup analysis also indicated that SWIM achieved some success in working with Hispanics – a group for whom impacts in welfare-to-work programs generally have been small and not statistically significant in the few studies that have included them. Hispanic experimentals showed increases in education and training (relative to the Hispanic controls) that were substantially greater than the increases for non-Hispanic whites or blacks (not shown in the tables). Tables ES.3 and ES.4 show that earnings gains among Hispanics were above average for both AFDC-FGs and AFDC-Us, although impact differences across ethnic subgroups were not statistically significant. Hispanic earnings gains in year five were not much below those in year two, indicating a relatively stable pattern over time and suggesting that additional earnings impacts may accrue after the five-year follow-up period. Hispanics also obtained AFDC reductions, which were statistically significant for AFDC-FGs but not for AFDC-Us. AFDC impact differences across ethnic subgroups were statistically significant for AFDC-Us but not for AFDC-FGs.

- **The distribution of SWIM's earnings gains and AFDC reductions across subgroups suggests the value of working with a broad spectrum of the eligible population, from the most "job ready" to the most "disadvantaged."**

Many welfare-to-work programs, when faced with limited resources, have targeted their services to certain subgroups in the eligible population. In the Work Incentive (WIN) Program, which was in place from the 1960s till the late 1980s, local operators often targeted services to those deemed "most employable." In the JOBS program, financial incentives encourage program operators to serve long-term AFDC recipients. SWIM's subgroup impacts are consistent with the targeting conclusion reached in an earlier study of subgroup impacts:[13] Particularly where reductions in AFDC payments are a program priority, the evidence favors working with a broad spectrum of eligible individuals without applying narrow selection criteria or subjective screening techniques.

SWIM's subgroup findings, however, are also somewhat surprising in light of the earlier subgroup study, which would have led one to expect below-average earnings impacts for the more "job ready" subgroups. This was not the case in SWIM. Instead, SWIM's earnings impacts were generally larger for the more job ready. The fact that no single group invariably showed the best results across subgroup studies provides further reason to avoid targeting plans that focus exclusively on one narrow set of subgroups.

- **The ratio of earnings gains to AFDC reductions for a number of the relatively disadvantaged subgroups was low, leaving those subgroups worse off financially, at least in terms of their own earnings and AFDC income.**

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[13] Friedlander, 1988.

Earnings impacts were not highly correlated with AFDC impacts across subgroups. Subgroups with relatively large earnings gains were often not the ones with relatively large AFDC reductions. Furthermore, as shown in Tables ES.3 and ES.4, for both AFDC-FGs and AFDC-Us, earnings gains during the five-year follow-up period were less than corresponding AFDC payment reductions for individuals without recent earnings, without a high school diploma or GED, with more than two years on their own AFDC case, and for "more disadvantaged" recipients. This finding reinforces the importance of developing ways to increase earnings impacts for the most disadvantaged.

- **Although net program costs were highest for the more disadvantaged subgroups, even they produced savings for government budgets that fully covered their net costs.**

The subgroup analysis also investigated whether the net costs of SWIM were higher for the more disadvantaged subgroups and, if so, with what consequence for government budgets. To conduct this analysis, the benefits and costs of the program were compared for the first-time AFDC applicants, returning applicants, less disadvantaged recipients, and more disadvantaged recipients (the "level of disadvantage" dimension shown in Tables ES.3 and ES.4). The results (not shown in the tables) indicated that, for the most part, the greater the level of disadvantagedness, the higher the cost. However, government budgets broke even for all subgroups except first-time AFDC-U applicants. The highest net costs were for AFDC-FG "more disadvantaged" recipients and AFDC-U "less disadvantaged" recipients. These two groups produced relatively low government savings, but savings nonetheless.

Conclusions

The SWIM study provided an opportunity to examine the effects of a pre-JOBS welfare-to-work program over an unusually long (five-year) follow-up period. The study's time frame, along with its extensive data on subgroups and participation, allowed new issues to be addressed. In addition, since San Diego SWIM remains an important example of a saturation approach, implemented at large scale in a major inner-city area by an experienced and nationally recognized local agency, the evaluation findings can inform decisions about designing JOBS programs and the more general debate around welfare reform.

Extending the follow-up three years beyond the original end date for the demonstration did, however, raise issues of interpretation because a substantial proportion of control group members eventually were subject to the participation requirements of GAIN, California's JOBS program, which replaced SWIM after the demonstration period. Because SWIM control group members were exposed to GAIN, the earnings gains and AFDC reductions reported for the latter part of the five-year follow-up period probably underestimate the magnitude and duration of the impacts possible under a permanent SWIM program.

A number of conclusions nevertheless appear well supported by the empirical results. The SWIM program had three fundamental goals: (1) It sought to change the nature of AFDC by imposing a serious requirement that eligible people participate in an employment-focused program. Through this, it aimed to (2) increase employment and (3) reduce AFDC costs. The program was successful in meeting the first goal: conditioning receipt of full AFDC benefits on continuing participation. SWIM achieved its other two objectives by moving people into jobs and off AFDC sooner than would otherwise have been the case, resulting in substantial savings of government funds. SWIM produced impacts on earnings and AFDC payments for a variety of subgroups. These subgroup findings are consistent with SWIM's broad, inclusionary targeting approach and do not support targeting such a program exclusively on any particular subgroup. SWIM was less successful in achieving goals often stated for other programs but not explicitly sought in SWIM. SWIM did not lead to better jobs and did not increase total family income, at least as far as the study was able to measure total income. Even with SWIM, large numbers of enrollees remained jobless and on AFDC. Moreover, the subgroup results also suggest that the greatest challenge for JOBS will be to find program approaches that can more effectively increase the earnings of the most disadvantaged AFDC recipients.

CHAPTER 1

OVERVIEW

This report presents five-year follow-up results for the Saturation Work Initiative Model (SWIM) in San Diego, which operated as a demonstration program from July 1985 through September 1987. The SWIM demonstration, sponsored by the U.S. Department of Health and Human Services (HHS), tested the feasibility and effectiveness of requiring ongoing participation in employment-directed activities by the maximum possible proportion of the then-mandatory Aid to Families with Dependent Children (AFDC) caseload. In its aim to thus "saturate" the caseload, SWIM went beyond typical previous welfare-to-work programs or typical current Job Opportunities and Basic Skills Training (JOBS) programs. SWIM was a mandatory program in that enrollees who did not comply with their program assignments could be sanctioned with a partial, temporary AFDC grant reduction, and that sanctioning authority was, in fact, exercised to a significant degree. SWIM was a broad-coverage program in that it attempted to reach all eligible individuals in its target area,[1] two inner-city districts of San Diego. Unless already employed or in a qualifying activity, new enrollees in SWIM were required to participate in a fixed sequence of activities, beginning with job search assistance, followed by assignment to unpaid community work positions, and then to education and training in the large San Diego Community College system and at other local providers. Over a period of two years, SWIM enrolled more than 10,000 individuals. With gross operating costs around $1,500 per enrollee, San Diego SWIM was among the more costly of the large-scale welfare-to-work interventions that have been studied in recent years. Table 1.1 summarizes the key features of SWIM.

The five-year findings from the SWIM evaluation arrive at an opportune moment to inform the national welfare reform debate. SWIM represents one approach to moving welfare recipients into jobs and to changing the nature of welfare by establishing a requirement to work or prepare for work. In its use of monthly participation targets and its inclusion of education services in its program model, SWIM anticipated the JOBS provisions of the Family Support Act of 1988. At a time when

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[1] Persons eligible for SWIM on a mandatory basis were those who were mandatory for the Work Incentive (WIN) Program under the rules in effect prior to the Family Support Act of 1988. All able-bodied two-parent (AFDC-Unemployed Parent or AFDC-U) case heads were mandatory. Single-parent (AFDC-Family Group or AFDC-FG) case heads were mandatory if their youngest child was at least age six.

TABLE 1.1

KEY FEATURES OF THE SATURATION WORK INITIATIVE MODEL (SWIM) DEMONSTRATION

Purpose	To test the feasibility and effectiveness of requiring ongoing participation in employment-related activities by a high proportion of the welfare caseload.
Location	Two of seven welfare employment offices in the County of San Diego, California. These two offices served the most urban and disadvantaged part of the county's AFDC caseload.
Targeted Population	Individuals applying for or receiving benefits under the Aid to Families with Dependent Children (AFDC) program. Participation was mandatory for the approximately 40 percent of the caseload required by pre-1988 federal law to participate in the Work Incentive (WIN) program. This segment of the caseload was comprised of single, generally female, heads of households with children age six or older (AFDC-FG cases) and case heads, usually male, of two-parent households (AFDC-U cases).
Program Model	Fixed sequence of activities, during which individuals could find employment and/or leave welfare at any point. Individuals were usually first assigned to a two-week job search workshop. Those who had not found jobs by the time they completed the workshop were assigned to three months of unpaid work experience as well as biweekly job club sessions. Those still unemployed after work experience would be assessed and referred to community education and training programs (SWIM itself did not operate these programs).
Dates of Operation	July 1985 through September 1987.
Scale of Program	In a typical month during the demonstration's second year, about 3,600 individuals were registered and eligible for SWIM.
Key Contextual Factors	Strong local labor market; relatively high state welfare grant level; broad network of education and training facilities, particularly community colleges; extensive experience on the part of the Department of Social Services in successfully implementing welfare employment programs.
Funding	County's regular WIN allocation from the federal and state governments, supplemented by special state monies and by federal demonstration funds.
Research Design	Random assignment of program enrollees to an experimental group, whose 2,290 members were required to participate in SWIM, or to a control group, whose 2,260 members were not eligible for SWIM activities but could, on their own initiative, enroll in other community programs. In addition, sample members randomly assigned plus 5,991 more SWIM enrollees were tracked to study participation in the program.
Impact Follow-Up	Five years from the date each research sample member enrolled in SWIM, covering a period from July 1985 through September 1991.
Key Impact Measures	Comparison of the employment, earnings, and welfare receipt of individuals in the experimental and control groups.

impact results are not yet available for JOBS programs, the SWIM demonstration stands as an example of one approach to building a JOBS program, an approach emphasizing rapid job entry, broad coverage, and an ongoing participation requirement.

The SWIM approach is not the only approach to JOBS; nor is it the one that has been most commonly adopted. Many JOBS programs emphasize skills-building, with initial assignments to education and training instead of job search, and do not necessarily emphasize quick labor market entry.[2] This type of program approach generally provides in-depth assessments of enrollee needs and capabilities as an initial program activity, and is often also characterized by greater enrollee voluntarism and choice of subsequent activities, non-universal enrollment practices, and the targeting of program resources on a narrow portion of the eligible caseload.[3] Among other things, in placing education and training at the end of the participation sequence for most enrollees, SWIM differs from the common JOBS practice of assigning education and training as a first activity for large numbers of enrollees for whom program staff deem it appropriate.

The Manpower Demonstration Research Corporation (MDRC) conducted the original two-year follow-up study of SWIM under a contract with the California State Department of Social Services, with support from the U.S. Department of Health and Human Services (HHS). The five-year study of SWIM was funded by HHS as part of its JOBS evaluation.

This report – the third on the SWIM demonstration – presents five-year impacts on employment, earnings, and AFDC receipt, as well as a comparison of program benefits and costs. The report updates the impact and benefit-cost findings of Hamilton and Friedlander (1989), which were based on only two years of follow-up data for earnings and AFDC payments. A separate paper is being prepared to update the SWIM participation analysis (Hamilton and Friedlander, forthcoming).

Results here are produced separately for the single-parent, mostly female, AFDC-FG assistance category and the two-parent, mostly male, AFDC-U assistance category.[4] Single parents with a child under age six were generally exempt from mandatory participation in SWIM, whereas all able-bodied

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[2] See Hagen and Lurie (1992) for a description of JOBS programs in 10 states.

[3] See Friedlander and Burtless (forthcoming) for some comparison of SWIM results with those for a more choice-oriented program run in Baltimore during the 1980s.

[4] The designation "AFDC-FG" is used for the single-parent AFDC category in conformity with practice in California and in order to allow "AFDC" to stand as the generic term comprising both AFDC-FG and AFDC-U.

AFDC-U cases could be required to participate. The behavior of AFDC-Us is expected to be different from that of AFDC-FGs for several reasons. AFDC-U cases generally received larger welfare payments than AFDC-FG cases because the needs of the second parent were figured into the monthly grant amount. AFDC-Us, however, were subject to tighter eligibility requirements and more stringent sanctioning penalties. According to regulations in effect during the SWIM demonstration, eligibility for AFDC-U terminated when the case head worked more than 100 hours per month, regardless of earnings. In addition, a sanction closed an AFDC-U case rather than merely reducing the grant temporarily as it did for AFDC-FG enrollees.[5] Also, as heads of two-parent families, AFDC-U enrollees did not have child care constraints on program participation and employment to the same degree as those for the single-parent AFDC-FG registrants. Finally, above and beyond the gender differences, a number of other demographic differences distinguished the two assistance categories. For example, AFDC-Us were more often Hispanic or Asian and had lower rates of high school completion, but they had higher rates of recent prior employment and much shorter AFDC histories than AFDC-FGs.

The impact estimates presented here are based on an experimental design, with random assignment of individuals to experimental and control groups occurring at the point of enrollment in SWIM.[6] Experimental sample members were enrolled in SWIM and were subject to SWIM participation requirements; controls were not eligible for SWIM services or subject to the requirements and thus represent individuals' experiences in the absence of SWIM. The estimates of SWIM impacts are the differences in employment, earnings, and AFDC outcomes between experimentals and controls. Prior research suggests that experimental group members may be expected to experience widely differing effects from SWIM. For some, labor market and welfare behavior probably changed dramatically; outcomes for many others quite likely remained unchanged. The estimates in this report represent averages of these various individual responses. The impact estimates, i.e., the experimental-control differences, are usually expressed as "increases in earnings" (average earnings in the experimental group compared to average earnings in the control group) or "decreases in welfare receipt" (average welfare receipt levels for experimentals compared to those of controls). Where we make comparisons between SWIM and other programs, we refer only to findings for other broad-coverage, experimentally evaluated, welfare-to-work programs.[7]

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[5] There was an exception to this rule. The State of California made AFDC-U sanctioning penalties for noncompliance with unpaid work assignments – one type of SWIM activity – the same as those for AFDC-FGs.

[6] Individuals were enrolled in SWIM in one of three ways. First, persons applying for AFDC who were found to be mandatory for SWIM were referred to SWIM by the Income Maintenance office. They were scheduled for an enrollment interview at the SWIM office and told to report at the appointed time and place. Second, persons who were on AFDC but, during the usual review of case status, were found to have become mandatory were referred to SWIM. Third, persons who were on AFDC and were already mandatory at the time the SWIM demonstration began were referred to the program for enrollment over the course of the first year of the demonstration.

In considering the SWIM results, it is important to keep in mind that welfare-to-work programs have a variety of possible goals. Some of these goals may be in conflict, and some are assigned more or less weight by different policymakers. Possible goals include: imposing a participation requirement on those targeted for the program; increasing overall employment levels among AFDC recipients; reducing the overall level of AFDC receipt; reducing the level of AFDC receipt particularly among long-term or potential long-term AFDC recipients; increasing individuals' self-sufficiency (e.g., increasing the proportion of income that individuals receive from earnings and decreasing the proportion obtained through AFDC); saving money for government budgets by reducing AFDC and other welfare expenditures; increasing the employment of AFDC recipients in high-wage, full-time, stable jobs; making families better off financially regardless of the sources of their income; and reducing poverty. Goals emphasized by the SWIM program designers and administrators included imposing a participation requirement on the entire then-mandatory AFDC caseload, increasing overall employment levels, and reducing AFDC receipt. The report examines the extent to which SWIM met these goals as well as other possible goals.

As discussed in this report, on average, SWIM experimentals in both AFDC-FG and AFDC-U assistance categories experienced five-year impacts on earnings and AFDC that would be judged large in the context of similar experimental evaluations. Permanent impacts on earnings and impacts on long-term welfare receipt, extending beyond the five-year mark, appear likely to be small, however. The bulk of impacts on earnings were associated with increases in the amount of time experimental sample members were employed rather than with higher rates of pay while working. AFDC reductions appeared to be primarily the result of fewer months on AFDC, although a significant fraction of savings in the AFDC-U category may have resulted from lower monthly grant amounts for sample members who remained on AFDC. Based on a variety of financial effects taken into account in a benefit-cost framework, AFDC-FG sample members approximately broke even, with gains in earnings offset by losses in welfare income; AFDC-U sample members showed a modest net loss. Government budgets accrued substantial gains because savings connected with welfare reductions greatly exceeded net program costs. Overall, for every dollar spent on SWIM, government budgets saved approximately $2.30 for AFDC-FGs and $2.40 for AFDC-Us.

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[7] Reviews of results for other programs may be found in Friedlander and Gueron (1992) and Gueron and Pauly (1991).

This report moves beyond the previous report (Hamilton and Friedlander, 1989) by addressing the question: How long will a typical group of SWIM enrollees maintain higher earnings and lower AFDC receipt than controls? In looking for a definitive answer, one is hampered to some degree by the original design of the evaluation, which envisioned a two-year, not a five-year, follow-up. Under the original design, the control group was kept out of program services for two years after the last sample member was randomly assigned. Shortly after that time, controls could begin to receive services from California's Greater Avenues for Independence (GAIN) Program, SWIM's successor program and California's current JOBS program. Through a special study of SWIM sample members' activity in GAIN – described in Chapter 3 and Appendix A – it was determined that both experimentals and controls did, in fact, participate to a significant degree in the latter part of the five-year follow-up, after the original end date of the SWIM experiment. The rate of participation in GAIN was similar for SWIM experimentals and controls among AFDC-FGs; among AFDC-Us, SWIM controls actually participated somewhat more in GAIN than experimentals. There is no easy way to determine what effect this situation had on the behavior of experimentals and controls in the later years of the five-year follow-up period analyzed in this report. Possibly, the impacts past the two-year follow-up point would have been different had SWIM been a permanent program, i.e., if experimentals had been subject to SWIM, and not GAIN, in the later follow-up years. Very probably, impacts past the two-year follow-up point would have been higher had controls not been exposed to GAIN or assigned to its program components.[8]

Nonetheless, the question of long-term impact is important. For each extra year that a group of experimentals stays ahead of controls, that year's difference between the two groups is added to the cumulative total program impact. But it is often argued that the initial effect of employment and training services on any group of enrollees will begin to "wear off" over time, that experimental sample members will begin leaving the jobs they found and returning to AFDC. In that case, the experimental-control difference, which is the estimate of program impact, will start to narrow. Each year's addition to the total impact for the group will become smaller and smaller. Wearing-off of the program effect is associated with the term "impact decay," which is widely used to describe the narrowing of the experimental-control differential over time.

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[8] This difficulty in estimating longer-term impacts does not exist for evaluations of time-limited programs such as the National Supported Work Demonstration, which provided individuals with approximately one year of work experience under conditions of gradually increasing demands, close supervision, and peer support, or the JOBSTART Demonstration, which provided basic education, occupational skills training, support services, and job placement assistance to high school dropouts over an average period of time of seven months. These programs were intended to provide services to individuals for only a short period of time. Furthermore, after the period of program treatment, experimentals and controls were expected to receive other types of program services. In SWIM, while the demonstration was time-limited, the theory behind the program assumed an ongoing program treatment, i.e., participation in SWIM for as long as an individual was receiving AFDC.

Wearing-off is not the only possibility, however. In fact, long-term patterns may differ for earnings and AFDC outcomes. Past research has shown that AFDC receipt is dynamic: Even without special welfare-to-work programs, AFDC grants are terminated as case heads become married or reconciled, or find jobs, or as their children grow up past the age of AFDC eligibility. Thus, a program such as SWIM may initially produce more rapid AFDC case closures among a group of experimentals, but eventually there will be case closures among controls as they begin to "catch up" to the experimentals. Over time, the natural dynamics of welfare receipt must result in a *convergence* of AFDC receipt rates for experimentals and controls at zero. Consequently, the experimental-control difference must sooner or later go to zero, too. Under this kind of catch-up scenario, the narrowing over time is not the result of any wearing-off of program effectiveness. Rather, it is the result of the inevitable end of welfare receipt for all sample members. In actuality, wearing-off and catch-up may both contribute to the time pattern of impacts, perhaps in equal measure or perhaps with one or the other dominating. To allow for all possibilities, this report uses the more neutral term "convergence" rather than "decay" in describing the narrowing.

For earnings, the situation is more complicated. There is no fixed ceiling that experimental and control earnings must eventually reach. Convergence need not occur. In theory, it is possible for experimentals to maintain a permanent lead. It is quite likely, however, that as controls begin to find jobs on their own, they will appear to catch up to experimentals to some degree, resulting in some decrease in the experimental-control differential. (This is particularly likely to be the case in SWIM's five-year impacts because controls, as noted above, were exposed to the GAIN program and assigned to its services during the third, fourth, and fifth years of follow-up.) Again, the narrowing may occur without the program effect wearing off. Any of a variety of patterns of growth, narrowing, and persistence is possible; and the pattern for earnings impacts may differ from that for AFDC impacts. Above all, it should also be noted that earnings impacts and AFDC impacts that accumulate for a group of enrollees before the experimental-control differential begins to narrow remain real and do not disappear. It is only the *additions* to cumulative impacts that become smaller with time or stop altogether if the experimental-control gap narrows all the way to zero. Furthermore, narrowing of the impact differential after several years for one group of enrollees does not mean the next group of enrollees will not experience program effects. In fact, the next group of enrollees – and each succeeding group – should exhibit the same pattern of impact growth and decline over time.

At year two, which was previously the limit of available follow-up, the measured differences between SWIM experimentals and controls were large, but it was not clear how long these would persist. In particular, it was an open question whether the earnings differential between experimentals and controls would narrow over time. It was deemed possible that SWIM might have conferred upon experimentals a permanent advantage in earnings over the control group. If so, then the additional follow-up data might have shown an improvement in the five-year net financial results for enrollees. In fact, the additional data did not show large permanent earnings differences but, rather, a combination of catch-up with some wearing-off for impacts on employment and earnings, combined with the expected catch-up for impacts on AFDC. Financial effects on AFDC-FGs are, in fact, quite close to those projected previously over a five-year horizon using the then-available two years of follow-up. Financial effects on AFDC-Us appear somewhat more negative using the actual data for years three, four, and five instead of projections. In addition, savings for government budgets, though still large, are not as large as previously projected. Nevertheless, the earlier conclusions still hold: SWIM produced large net financial benefits for government budgets but did not improve the financial position of the SWIM enrollees (although they obtained more of their income from employment and less from welfare).

The time pattern of impacts is of critical importance in understanding the workings of a program such as SWIM.[9] Permanent impacts on earnings and impacts on long-term AFDC receipt, extending beyond the five-year mark, appear likely to be small. As a consequence, additions to total program impact after year five are also likely to be small. These findings do not imply that the program was ineffective. Rather, the constellation of results from SWIM indicate that SWIM achieved its objectives in a manner consistent with its emphasis on rapid employment. It appears that the main SWIM effect on earnings came about through an acceleration of job-finding activity. Individuals who would have found jobs on their own found them faster under SWIM, and some individuals who would not have worked at all during the five-year follow-up did so. But the jobs found were similar to those found by controls. Earnings per quarter of employment were similar, and there remained a high degree of employment instability among experimentals. There was little evidence of any sustained boost to earning power, such as might be expected from basic or occupational skills development.

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[9] A more detailed analysis of the time pattern of impacts for SWIM and three other welfare-to-work programs may be found in Friedlander and Burtless (forthcoming).

The results summarized above pertain to impact estimates for the full AFDC-FG sample and the full AFDC-U sample. Impacts were also estimated separately for a number of subgroups within the AFDC-FG and AFDC-U samples. The first objective was to see whether these subgroup results could provide a guide for targeting of welfare-to-work program resources in the future. In several (but not all) respects, the findings are consistent with previous work on subgroup impacts, which examined only AFDC-FGs.[10] In general, there is evidence of impacts on earnings and AFDC payments for a variety of subgroups. The findings do not support exclusive program targeting on any particular subgroup. Particularly when AFDC savings are a high priority, a broadly inclusive targeting approach appears warranted, with attention given to the more disadvantaged subgroups as well as the most "job ready." As found in earlier work, however, the ratio of AFDC reductions to earnings gains for the more disadvantaged is often high. Indeed, in SWIM, AFDC reductions exceeded earnings gains for several subgroups.

In other respects, the SWIM subgroup results differ from prior subgroup impact work. Among AFDC-FGs, above-average earnings gains and usually at least average AFDC reductions, relative to the full sample impacts, were found for various subgroups of the most job ready, least disadvantaged SWIM enrollees. Among AFDC-Us, some of these SWIM subgroups also showed above-average earnings gains, although the result was not as consistent as for AFDC-FGs and was not matched by even average AFDC reductions. In prior work (on AFDC-FGs), these subgroups usually showed below-average or small impacts. At the same time, the AFDC-FG subgroup of applicants who were returning to AFDC after a spell off, which in previous subgroup work had shown above-average earnings gains, in SWIM showed none. Finally, AFDC-FG sample members who were already receiving AFDC at the time of enrollment in SWIM evidenced above-average earnings gains, which was not typical of earlier estimates.

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[10] Subgroup results in the present study were compared with those of Friedlander (1988), who examined subgroup impacts in five experimental evaluations of welfare-to-work programs of the 1980s.

A question often asked in evaluation research is: What elements of the program produced the observed effects? As part of the subgroup analysis, evidence concerning the underlying mechanisms of effect for SWIM was sought. In theory, a variety of mechanisms may have been at work: increased earning power gained through participation in program activities, removal of barriers to employment, changed attitudes toward welfare and work, sanctioning, and others. The empirical analysis was by no means exhaustive, and conclusions about causality are subject to considerable uncertainty. The analysis should be viewed more as suggesting additional avenues for study than as providing definitive answers.

For evidence that participation in SWIM activities was helping enrollees in the labor market, there was an examination of subgroups with the largest experimental-control differences in (1) rates of participation in any activity and (2) average days enrolled in education and training. The aim was to see whether groups with large experimental-control differences in these activity measures were also groups with above-average impacts on earnings. However, a strong link could not be documented between increased participation in program activities and program impact. Rather, only a weak correlation was found across subgroups between increased activity and earnings impacts. For example, subgroups facing relatively long future AFDC spells and relatively poor prospects for employment without program assistance often had the largest increases in participation in job search, unpaid work assignments, and education and training as a result of SWIM. But these groups did not generally have the largest impacts on earnings, suggesting that the effectiveness of the various SWIM activities may have been lower for longer-term, more disadvantaged AFDC recipients. This does not mean that it was not worthwhile to serve these subgroups. It may mean that the cost of success could be larger for them. In fact, the cost estimates did appear higher for the more disadvantaged subgroups. But a benefit-cost analysis of selected subgroups indicated that government budgets at least broke even for nearly all subgroups, including the most disadvantaged.

The mechanism of AFDC effects also appears to be complex. Only a weak correlation was found between impacts on earnings and impacts on AFDC payments across subgroups. Subgroups with large (or small) impacts on earnings are often not those with large (or small) impacts on AFDC payments. Nor was a strong connection found across subgroups between higher sanctioning rates and larger AFDC impacts. The threat of sanction may or may not have been vital to securing the cooperation of enrollees, but the direct contribution of sanctions to AFDC savings appears to have been limited. These weak empirical relationships suggest that, in addition to earnings gains and sanctioning, some other mechanism also contributed to AFDC reductions. It appears that for some enrollees, the effect of SWIM – through its participation requirement and through the program "message" – was to make continued AFDC receipt seem less attractive relative to alternatives. These alternatives could include finding a job, but they also might include income from contributions of the absent parent or other family members or from off-the-books (unreported) employment, i.e., income not captured in Unemployment Insurance records, the source of earnings data in this study.

In assessing the SWIM results, the greatest uncertainty concerns effects at the end of the five-year observation period. Because the SWIM experiment was time-limited, its results cannot represent the long-run costs and effects that might be obtained for a group of enrollees if SWIM had been a permanent program. In a permanent program, enrollees still on AFDC after two or three years, or those returning after having exited, would have remained subject to the same participation mandate. To a considerable extent, experimental sample members did continue their participation, not in SWIM but in GAIN, although the latter was a different program. But a control group used to estimate the effects of a permanent program would have had to have been excluded from SWIM, GAIN, or any other welfare-to-work programs indefinitely. The data on participation in GAIN indicate that this permanent exclusion did not occur for SWIM controls. Moreover, even had an ongoing control exclusion been possible, a short-term SWIM experiment would not have captured potential long-run reductions in applications to AFDC that might have occurred once the program participation requirement became permanently institutionalized and well known in the community.

What would the long-run effects of a permanent SWIM program have been? It is only possible to speculate. It may be that prolonged and repeated exposure to the SWIM treatment for a group of AFDC case heads could produce additional effects several years following the initial SWIM experience, perhaps enough to create a longer-lasting increase in employment and earnings and additional AFDC savings for government budgets. In fact, discussion in the report suggests that the third-, fourth- and fifth-year impacts analyzed in this study probably underestimate the impact of SWIM as a permanent program. If SWIM had operated for the full five-year period and if control group members had been prevented from receiving GAIN services throughout this same period, it is likely that control group "catch-up" would have been slower, resulting in a greater and longer-lasting experimental-control difference in earnings and AFDC receipt in the later follow-up years.

More generally, this issue suggests that more research should be done to determine how to estimate the magnitude of the long-run effects of welfare-to-work programs, given the frequency with which these programs change over time and the ethical and political issues raised by preventing control group members from receiving program services for more than a few years.

With careful attention to these issues, extending the SWIM follow-up from two years to five can provide important information for emerging JOBS programs, addressing the following JOBS-relevant questions: What is a lower-bound estimate for the magnitude and duration of the effects of a saturation program that placed a heavy emphasis on job search and unpaid community work experience? What are the five-year patterns of welfare receipt and employment among experimentals and controls? What is the pattern of control group "catch-up" over a five-year follow-up period? What are the five-year patterns of experimental and control behavior for various subgroups within the program-mandatory part of the AFDC caseload? Do these patterns vary by subgroup? What are the implications of these subgroup findings for the targeting of services within welfare-to-work programs? Can a program such as SWIM result in individuals obtaining higher-paying jobs over a five-year period? Can a SWIM-like program save money for taxpayers over five years?

The remainder of the report is organized as follows. Chapter 2 considers several hypotheses concerning the mechanisms through which SWIM might have achieved impacts. Chapter 3 describes the differences in participation and sanctioning between experimentals and controls. Chapter 4 presents the differences in outcomes between experimentals and controls, which constitute estimates of the impacts of SWIM. Chapter 5 applies benefit-cost accounting methods to the program effects. Chapter 6 concludes with an analysis of participation and impacts for important subgroups within the research sample.

The methodology for producing impact and benefit-cost estimates for this report is the same as for the previous report (Hamilton and Friedlander, 1989). Details of the estimation techniques and analysis issues may be found in that volume and are, for the most part, not repeated here. For the two-year period of overlap in the follow-up, there exist some small discrepancies in estimates between this and the previous report. These result from a small amount of updating or correcting of historical earnings and AFDC payments in automated systems over the past three years. In addition, one person was dropped from the AFDC-FG sample and one from the AFDC-U sample.[11]

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[11] These were individuals recently discovered to have been duplicated on the data file.

CHAPTER 2

MECHANISMS UNDERLYING SWIM IMPACTS

The primary function of the experimental design in the SWIM evaluation is to produce reliable and internally valid estimates of the basic program impacts on employment, earnings, and AFDC receipt. The randomization process can rigorously establish that the program, and not some other influence, was the causal factor in producing the estimated changes in behavior. The fundamental experimental-control comparison cannot, however, directly tell us much about *how* SWIM worked, about the underlying mechanisms whereby behavior was changed. If we wish to open the "black box" and learn more about the relative importance of the various elements of SWIM, we must go beyond the basic experimental design and consider other methods to apply to the data.

The discussion begins by considering several hypotheses about how SWIM might have achieved impacts. Rather than focus on particular program activities such as group job search workshops or unpaid work assignments, the hypotheses posit general mechanisms that may have been effective in several activities or through case management or counseling. The aim is to describe alternative mechanisms by which the program could have changed behavior, to analyze the differences in effects that each might have produced, and then to determine the behavior patterns that might have resulted if one or the other mechanism were dominant. With the full array of empirical results before us, it will be possible to decide which hypotheses they support and which they do not. The method, then, will be to compare the observed pattern of results with the pattern expected if each mechanism were dominant to see which mechanisms fit the best. This method may not, of course, be able to pin down with certainty which mechanisms were effective, especially if several were operating at once. The posited mechanisms and their behavioral consequences are as follows:

1. **Increased Earning Power**. A relatively permanent increase in earning power might be expected from basic and occupational skills development, from education or training credentials, from increased information about labor market opportunities, from assistance looking for an attractive job, or from placement in scarce high-wage jobs or jobs with high-wage growth prospects. Increased earning power would then lead to higher actual earnings, which, in turn, would result in reduced AFDC receipt. The reduction in AFDC comes about because the enrollee judges the value of the increased

future earnings as high enough to compensate for the loss of AFDC and associated welfare benefits and therefore elects work instead of welfare.

Empirically, one would expect to observe persistent impacts on earnings; an increase in the number of enrollees earning in the higher brackets; a positive correlation between participation in services and earnings impacts; a positive correlation between earnings gains and AFDC reductions across subgroups within the research sample; a relatively high ratio of earnings gains to AFDC reductions; and a positive total value of earnings gains less AFDC reductions and other transfer effects, constituting a net financial improvement for the program enrollee.[1]

2. Removal of Barriers to Employment. Assistance in acquiring child care and provision for transportation expenses may remove some constraints on employment. More broadly, reduced anxiety about working, which might come from counseling, the encouragement and guidance obtained in a job search workshop, a work experience assignment, or a first job would constitute another form of barrier removal. For example, under this scheme, when job search workshop leaders say (as they often do) that they try to "make people believe in themselves," they are, in effect, trying to remove a barrier to employment. Barrier removal may produce employment impacts, but it does not necessarily increase earning power.

Empirically, one would therefore expect to observe an increase in employment without necessarily an increase in earnings among employed program graduates. One would still expect to see a positive correlation between earnings gains and AFDC reductions across subgroups within the research sample; a favorable but not necessarily high ratio of earnings gains to AFDC reductions; and a positive total value of earnings gains less AFDC reductions and other transfer effects, constituting a net financial improvement for the program enrollee.

3. Increased Work Motivation. A change in attitude more toward favoring work over welfare would result in increased employment and reduced AFDC receipt. Such an attitude change might develop from counseling, the general "message" conveyed by the program, discussions in job search workshops, or new experiences in unpaid work assignments or on a new job. Under a pure attitude change scenario, earning power would remain unaffected. Again, AFDC reductions would occur only in response to employment.

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[1] There are circumstances in which earnings gains may be less than AFDC reductions under the increased earning power mechanism or the barrier removal mechanism (discussed below): e.g., if program enrollees so dislike being on welfare that low earnings are enough to "compensate" them for the loss of a larger amount of AFDC income, or if their earnings plus contributions from other family members exceed the value of staying on welfare.

Empirically, one would therefore expect to observe an increase in employment without necessarily an increase in earnings among employed program graduates. In fact, earnings per quarter of employment might actually appear lower if individuals decided to accept jobs that they had previously rejected. Nonetheless, one would still expect to see a positive correlation between earnings gains and AFDC reductions across subgroups within the research sample, since employment and earnings impacts lead to AFDC impacts. One might not see a high or even favorable ratio of earnings gains to AFDC reductions if work motivation becomes strong enough; and the total value of earnings gains less AFDC reductions and other transfer effects may or may not amount to a positive financial impact for the program enrollee.

4. **Increased Cost of AFDC Receipt Relative to Alternatives**. For some enrollees, a program "message," whether conveyed formally or informally, that welfare receipt should not be a way of life may increase any psychological stigma associated with AFDC receipt. For some, the demands of the program may add to the perceived burden of maintaining an AFDC grant, increasing the "hassle" of remaining on AFDC. These increases in the psychological costs of remaining on AFDC can change behavior, especially for people who have alternative sources of income, either because they can find work or because they can obtain some support from other family members. If alternatives to remaining on AFDC are available, some of these individuals will switch. In part, this may appear as more rapid job-finding, but some enrollees may also shift into non-work and non-AFDC status as they rely on contributions to income from an absent parent or other family members. AFDC reductions that occur may not always come in response to employment.

Empirically, one would expect to observe some increase in employment without necessarily an increase in earnings among employed program graduates. One would expect faster job-finding among enrollees who would eventually have found jobs anyway. Earnings per quarter of employment might appear lower if individuals decided to accept jobs that they had previously rejected. One would no longer necessarily expect to see a strong positive correlation between earnings gains and AFDC reductions across subgroups within the research sample. Nor would the ratio of earnings gains to AFDC reductions necessarily be favorable for enrollees; and the total value of earnings gains less AFDC reductions and other transfer effects might be close to break-even or even a net financial loss for some groups of program enrollees.

5. **Earnings Discovery**. Close contact between program enrollees and caseworkers may result in more rapid adjustment of AFDC grants in response to earnings changes, even without any program

effect on employment or earnings. The requirement to participate may force some enrollees who are working but are not reporting earnings to announce those earnings in order to be excused from participating. The earnings discovery mechanism does not produce any direct earnings effects, only AFDC reductions.

Empirically, one would expect to observe AFDC impacts without employment or earnings impacts. The ratio of earnings gains to AFDC reductions would be zero in the extreme case; and the total value of zero earnings gains less any AFDC reductions and other transfer effects would constitute a net financial loss for some groups of program enrollees.

6. **Sanctioning**. Participation requirements may create time conflicts for some enrollees. Enrollees with off-the-books (unreported) earnings may face a choice between continuing to work and satisfying program participation requirements. Or non-working mothers may wish to remain at home with their children instead of participating. For these individuals, their external activities impose an "opportunity cost" on their program participation, and some may choose to accept a financial sanction rather than participate. The application of financial penalties to enrollees may produce modest, temporary AFDC reductions. Such direct effects of sanctioning should not be confused with possible indirect effects of the threat of a sanction in securing compliance with program assignments.

Empirically, one would expect to observe AFDC impacts without employment or earnings impacts. For AFDC-FGs, one would expect AFDC impact to be associated with changes in monthly grant amounts and not from case closure, but one might not observe the same for AFDC-Us, for whom sanctioning rules were different. The ratio of earnings gains to AFDC reductions would be zero in the extreme case; and the total value of zero earnings gains less any AFDC reductions and other transfer effects would constitute a net financial loss for some groups of program enrollees.

This list of mechanisms and associated behavior patterns suggests several measures that should be added to the usual experimental-control impact differences in employment, earnings, and AFDC receipt. Also of interest are the distribution of earnings, average earnings per quarter employed, average AFDC payments per month in which such payments are received, and the four-way combination of employment and AFDC receipt status. For example, if employed experimentals have lower average earnings per quarter employed than do employed controls, this would argue against the importance of the earning power mechanism and for the importance of attitude change or other mechanisms.

This method implies a special role for subgroup analysis, i.e., for the computation of impacts and other measures for portions of the samples defined by characteristics observed at the time of random assignment. Subgroups may exhibit considerable variation in SWIM activity and in impacts. This variation may provide another opportunity to observe particular patterns of behavior that may constitute evidence favoring one or another mechanism of program effect. In particular, subgroup analysis allows one to examine the "correlation" between measures. Two measures that are positively correlated will both exhibit above-average values for a subgroup and below-average values for another. When measures are negatively correlated, subgroups that exhibit above-average values for one measure will tend to show below-average values of the other.

The usefulness of such correlations may be seen from some examples. For one, as indicated above, a high correlation might be expected between incremental participation and impact on earnings if the dominant mechanism of impact is increased earning power resulting from participation in services. If, instead, one finds earnings gains for groups with small incremental participation, then increased participation cannot be causing the impact for them. Ruling out a particular mechanism of effect in this case would be straightforward. In general, "disproving" hypotheses will be easier than "proving" them under this methodology.

With another pattern of results, the interpretation may not be as clear. For example, suppose one observes that certain subgroups with relatively high incremental participation rates have relatively low or only average impacts. It cannot be said with certainty that the incremental participation is not effective. It may be that the particular subgroups in question are more difficult to achieve impacts for. That is, one may be observing the effects of diminishing returns rather than evidence against the effectiveness of program participation.

Some mechanisms will be quite difficult to distinguish from each other with the data available. For example, the effects of increased work motivation will, for the most part, appear to be quite similar to those associated with the relative cost hypothesis. Both might produce increased employment with lower average earnings per quarter employed. It might be expected that the ratio of earnings gains to AFDC reductions will be greater for the increased work motivation, but theory does not provide guidance on what this ratio should be. A particular observed ratio may appear to be consistent with both competing hypotheses. To confuse the issue further, both the relative cost and sanctioning hypotheses would be consistent with a correlation between sanctioning rates and AFDC reductions.

This nonexperimental approach to investigating the internal mechanism of SWIM effects must be approached with some care, and conclusions based on it must be treated with caution. In the absence of random assignment to different activities and different participation requirements, it is not possible to establish causality with certainty. The patterns observed may be necessary but not sufficient for acceptance of a particular hypothesis. Alternative explanations will often be equally plausible. Nevertheless, under certain circumstances, it is possible to increase the understanding of how SWIM worked to achieve the basic impacts estimated from the fundamental experimental-control comparison.

Some additional discussion is warranted with regard to the status "not employed and not on AFDC," which may show an increase under the relative cost hypothesis. As will be seen later from the empirical results, a significant percentage of both experimentals and controls were in this status during the follow-up period. This status does not necessarily mean that the sample member did not have income. Contributions to income may have come from an absent parent, a new spouse, or other family members. In some cases, the family head also may have had off-the-book (unreported) earnings, and these may not show up in the follow-up data.[2] Finally, the family head may have moved to another state and, if employed, have had earnings not captured in California's Unemployment Insurance records.

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[2] Work by Jencks and Edin (1990) indicates that a significant proportion of income among welfare recipients may not be captured by welfare records or Unemployment Insurance reports. In interviews with 25 Chicago-area welfare recipients, the authors found that all 25 individuals supplemented their AFDC checks with income from other sources, that none reported all extra income to the welfare department, and that only two reported any of it. Among this small, not randomly selected group, individuals received approximately 57 percent of their income from AFDC or Food Stamps, 12 percent from jobs, 22 percent from absent fathers, boyfriends, relatives, friends, and student loans, and 9 percent from vice. It is likely that most of the income received from the last two categories would not be recorded in the data sources used in this five-year study; it is unclear how much of the income obtained through jobs might not be covered in Unemployment Insurance records; but probably all of the income received through the welfare system would be captured in the AFDC records used in this study.

CHAPTER 3

EXPERIMENTAL-CONTROL TREATMENT DIFFERENCES

In this chapter, participation and other activity measures for experimentals in the SWIM program are compared with the activities of controls during the same period. Welfare-to-work programs are often described with reference only to the activity of experimentals. But control group activity constitutes a baseline against which the program treatment must be assessed. On the one hand, to the extent that SWIM participation among experimentals exceeded the control baseline, SWIM participation should be expected to have contributed to the program's impact on earnings and AFDC payments. On the other hand, if certain activity estimates for controls approach those for experimentals, it is unlikely that those particular activities contributed much to the program's impact on earnings and AFDC payments. These experimental-control activity differences are thus critical in determining the actual "net" treatment of a program.[1] The qualifier "incremental" is used throughout this report to label such differences, and the SWIM treatment is discussed in terms of "incremental activity," "incremental participation," or "incremental treatment," meaning the difference in the treatment experiences of experimentals relative to controls. The robustness of the measures of incremental treatment sets this study apart from other studies containing estimates of program participation. In this study, detailed information is available on the activities of controls as well as those of experimentals, a situation not often encountered in studies of welfare employment programs. Consequently, there is a strong link between the SWIM participation and impact analyses: The comparison of experimentals and controls will give us both incremental treatment estimates and, in Chapter 4, estimates of program impacts.

This chapter also briefly examines participation by SWIM experimentals and controls in the Greater Avenues for Independence (GAIN) Program, which replaced SWIM in San Diego in September 1987 and subsequently became California's JOBS program. Once SWIM ended, experimental group members became eligible for GAIN. Starting in mid-1988, control group members also became eligible for GAIN. Since the five-year impact follow-up period examined in

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[1] The provision of program services is not the only defining feature of the net treatment of a program. The mandate to participate in a program and case management practices may, in and of themselves, be seen as part of the net treatment.

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this report captures SWIM's effects as well as possible effects from GAIN, the latter part of this chapter discusses experimental-control differences in receipt of GAIN services and their implications for interpreting the five-year impact estimates presented in this report.

A. Experimental-Control Treatment Differences Through June 1988

Estimates in this chapter pertain to activities occurring between the day an individual was enrolled in SWIM and the end of June 1988. Depending on the date a particular sample member enrolled in SWIM, these data cover a two- to three-year follow-up period. Throughout this report, sample members are defined as having participated in an activity if they started it, i.e., if they participated for at least one day. "Completing" an activity in a program such as SWIM does not have a clear meaning, since dropping out in the middle of an activity to take a job or leave welfare may be positive outcomes.

SWIM activity data were collected from two data systems maintained by the County of San Diego Department of Social Services: the SWIM Automated Tracking System (job search and sanctioning) and Employment Work Experience Program (EWEP) attendance logs (unpaid work experience). Two non-welfare data sources supplied education and training information: the San Diego Community College District Student Information System (education and training participation in adult schools and community college branches) and the San Diego County Job Training Partnership Act (JTPA) Management Information System (JTPA-funded job search assistance, education, and training).[2] Data in these latter two systems pertained to enrollments in basic education courses (adult basic education, GED [General Educational Development or high school equivalency] preparation, high school, English as a Second Language, and citizenship), college-level courses, and vocational training courses. Some of the education and training recorded in these latter two data bases resulted from placements or referrals of experimental group members by SWIM staff; much of it, however, was initiated by experimental or control group members on their own while they were on AFDC or after they had left the welfare rolls.[3]

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[2] The San Diego Community College District was the local district for about 90 percent of the SWIM research sample. The JTPA data base covered the entire County of San Diego.

[3] Participation data in this report are organized slightly differently from the data in earlier reports on SWIM. Among other things, for this report, the four data bases described in the text have been merged, permitting the grouping of activities under the general categories job search, unpaid work experience, education, and training to be made simpler than in Hamilton and Friedlander (1989). In addition, activity (continued...)

To summarize, few SWIM experimentals were not covered by the requirement to participate, work, or leave welfare. SWIM job search, unpaid work experience, and sanctioning all clearly contributed to the incremental treatment because many experimentals participated in these activities or received a sanction but almost no controls did so. Control group participation in education and training proved to be an important determinant of incremental participation, however. For example, more experimentals participated in education and training than in SWIM unpaid work experience, but the incremental participation rate for education and training was only half that of unpaid work experience. About two-thirds of the average enrollment days in education and training time for experimentals were offset by control group participation in similar activities.

Table 3.1 presents activity estimates during the first two to three years of the five-year follow-up period for experimentals and controls in the AFDC-FG and AFDC-U research samples. As shown, fewer than 1 percent of control sample members participated in SWIM job search and unpaid work experience, from which they were excluded under the research design.[4] Some 29 percent of the AFDC-FG controls and 23 percent of the AFDC-U controls participated in education or training.[5]

Counting all activities, 69 percent of AFDC-FG experimentals and 70 percent of AFDC-U experimentals participated during the two- to three-year follow-up period. These rates amounted to a 39 percentage point increase over the control participation rate for AFDC-FG experimentals and a 47 percentage point increase for AFDC-U experimentals. Job search participation rates exceeded

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[3] (...continued)
differences between experimentals and controls have been regression-adjusted using the same model as is used on the impact estimates. In Hamilton and Friedlander (forthcoming), activity differences were not regression-adjusted, and a small number of sample members with preschool-age children who were enrolled in SWIM because they were already participating in education and training were excluded. In this report, all sample members are included so that participation and impact samples will be identical.

[4] The few controls who participated in these activities or were sanctioned probably represent key-punching errors in the SWIM Automated Tracking System or individuals whom staff mistakenly treated as experimentals.

[5] Taking into account the differing follow-up periods, this level of activity among control group members appears to be higher than that found for control group members in a study of a Virginia welfare employment program, but similar to the activity rate calculated for control group members in a study of a Chicago program. In Virginia, within a 15- to 28-month follow-up period, 12.7 percent of the AFDC-FG control group members participated in public school, community college, or Job Training Partnership Act (JTPA) education or training programs (Riccio et al., 1986). In Chicago, within a 9-month follow-up period, 17.7 percent of the AFDC-FG control group members who attended a program orientation participated in education or training programs run by the community college or JTPA systems (Friedlander et al., 1987).

TABLE 3.1

SWIM

ACTIVITY ESTIMATES BY ASSISTANCE CATEGORY AND RESEARCH GROUP (JULY 1985 THROUGH JUNE 1988)

	AFDC-FG			AFDC-U		
Activity Measure	Experimentals	Controls	Difference	Experimentals	Controls	Difference
Participated in any activity (%) (job search, work experience, education, or training)	69.4	30.0	39.4 ***	70.2	23.2	47.1 ***
Participated in job search (%)	53.9	0.7	53.2 ***	59.5	0.8	58.7 ***
Participated in work experience (%)	21.4	0.7	20.7 ***	21.0	0.6	20.4 ***
Participated in education or training (%)	39.4	29.2	10.2 ***	33.5	22.6	10.9 ***
Average number of days enrolled	82.2	58.9	23.3 ***	66.5	42.5	24.0 ***
Average number of days enrolled per participant	*208.6*	*201.7*	*6.9 (a)*	*198.5*	*188.0*	*10.5 (a)*
Sanctioned (%)	11.4	0.1	11.3 ***	9.6	0.1	9.5 ***
Covered (%)	94.2	76.6	17.6 ***	96.3	83.4	12.9 ***
Sample size (AFDC-FG total = 3210) (AFDC-U total = 1340)	1604	1606		686	654	

SOURCE: MDRC calculations from the County of San Diego Department of Social Services SWIM Automated Tracking System and EWEP attendance logs; the San Diego Community College District Student Information System; and the San Diego County JTPA Management Information System.

NOTES: The sample for this table consists of individuals who registered between July 1985 and June 1986.

Activity measures are calculated as a percentage of the total number of persons in the indicated assistance category and research group. Follow-up begins at the point of initial registration, and ends June 30, 1988. This results in varying lengths of follow-up for each sample member. For example, individuals who registered in July 1985 have three years of follow-up while those who registered in June 1986 are followed for two years.

Participation is defined as attending a job search activity for at least one day, attending EWEP for at least one hour, enrolling in a community college program for at least one day, or attending a JTPA-funded activity for at least one day. Number of days enrolled is defined as the difference, in days, between the date an individual enrolled in a course or program and the date an individual completed or dropped out of the course or program. "Work experience" includes EWEP and OJT. "Education or training" includes basic education courses, college-level courses, and vocational training courses. Subcategory percentages may not add to category percentages because individuals can participate in more than one activity. Estimates are regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members. Rounding may cause slight discrepancies in calculating differences.

"Covered" is defined as ever participated in job search, work experience, or education and training, or ever was sanctioned, or ever became employed, or ever received no AFDC in a quarter. Participation and sanctioning pertain to the two- to three-year tracking period for those data for a sample member. Employment and AFDC were tracked for two years for each sample member for this coverage measure.

A two-tailed t-test was applied to differences between experimental and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

(a) Not an experimental comparison; statistical tests not performed.

50 percent[6] for AFDC-FG and AFDC-U experimentals, and unpaid work experience participation rates exceeded 20 percent for experimentals.[7] Virtually all the SWIM job search and unpaid work experience of experimentals was incremental participation. In contrast, much of the education and training undertaken by SWIM experimentals was not part of incremental participation. Some 39 percent of the AFDC-FG experimentals and 34 percent of the AFDC-U experimentals participated in education or training. These rates amounted to 10 and 11 percentage point increases, respectively, over control group participation in the same activities. Length of stay in these activities was also longer among experimentals. AFDC-FG and AFDC-U experimentals remained enrolled in education and training programs, on average, for approximately 23 and 24 days longer than their control counterparts (including those who never participated). Estimates pertaining only to *participants* within experimental and control groups are shown in italics in Table 3.1.[8] If we look only at participants, AFDC-FG experimentals who participated in education or training remained enrolled, on average, for seven days longer than the AFDC-FG controls who participated in education or training. Among the AFDC-Us, experimentals who participated in education or training remained enrolled, on average, approximately 11 days longer than participants in the control group.

Slightly over 11 percent of the AFDC-FG experimentals and almost 10 percent of the AFDC-U experimentals were sanctioned during the two- to three-year follow-up period. Since control group members were not assigned to any SWIM activities and therefore had no opportunity to fail to comply with assignments, their sanctioning rate was close to zero.[9]

The final activity measure shown on Table 3.1 is "coverage." This measure addresses the following question: To what extent did SWIM reach all experimentals with the mandate to participate, work, or leave welfare? Experimentals not covered would be those who remained jobless and on AFDC and escaped both participation and sanctioning during the first few years of the follow-up period. To examine the extent to which SWIM incrementally increased coverage, coverage rates were calculated for both experimental and controls. Individuals in the SWIM data are defined as covered if, within a two- to three-year follow-up period, they (1) participated in job search, unpaid work experience, or education or training; (2) were sanctioned; (3) became employed, as indicated by UI records; or (4) did not receive AFDC for at least one quarter, as indicated by AFDC records. Table 3.1 indicates that 77 percent of the AFDC-FG controls and 83 percent of the AFDC-U controls were "covered," solely through self-arranged participation in education or training, employment, or exits from AFDC during the follow-up period. Some 94 percent of the AFDC-FG experimentals and 96 percent of the AFDC-U experimentals were covered under SWIM, an 18 percentage point increase relative to controls for AFDC-FG experimentals and a 13 percentage point increase for AFDC-U experimentals.

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[6] Job search activities included two-week job search workshops and biweekly job clubs. Length of stay in job search was analyzed in Hamilton (1988). More than three-quarters of the job search workshop participants attended all 10 days of the workshops; on average, job club participants attended four job club sessions.

[7] On average, work experience participants worked 173 hours, or the equivalent of 25 full-time days, during the two- to three-year follow-up period (Hamilton and Friedlander, 1989). In addition to the unpaid EWEP work assignments, the "work experience" classification in this report includes a handful of subsidized on-the-job training (OJT) positions.

[8] Differences between participants are not true experimental comparisons, since nonparticipants have been dropped from the calculations.

[9] A sanction counted in the SWIM data (through June 1988) represents an actual reduction in an individual's AFDC grant, not simply a "request" to the Income Maintenance office that a grant reduction be initiated.

B. Experimental-Control Treatment Differences from July 1988 Through the End of the Five-Year Follow-Up Period

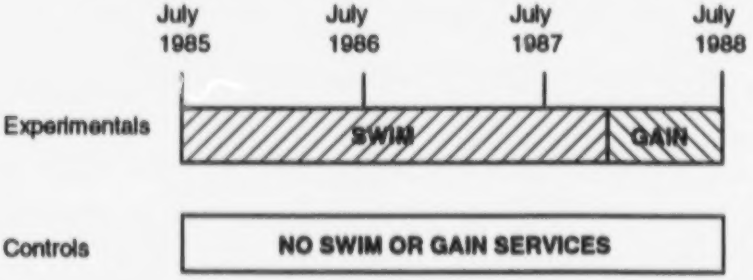
As noted earlier, the original design of the SWIM demonstration creates some difficulties in interpreting the five-year pattern of impacts presented in this report. In an ideal research situation, if the aim of the demonstration had been to measure the five-year effects of SWIM as a permanent program in San Diego, over and above services available regularly in the community, the program would have been operated for a full five years. In addition, individuals in the control group would have been excluded from any mandatory welfare-to-work program for the same five-year period. This design scenario is shown in Panel B of Figure 3.1. This was not, however, the design implemented. The original SWIM evaluation design envisioned a two-year, not a five-year, program and follow-up period. Accordingly, as shown in Panel A of Figure 3.1, the SWIM program was operated for only approximately two years (from July 1985 through September 1987), and SWIM control group members were kept out of mandatory welfare-to-work program services until two years after the last research sample member was randomly assigned (i.e., through June 1988).

After these dates (September 1987 for experimental group members and June 1988 for control group members), sample members in *both* research groups became eligible for the Greater Avenues for Independence (GAIN) Program, SWIM's successor program in San Diego and California's current, statewide, mandatory JOBS program. During the late 1980s, San Diego's GAIN program, like SWIM, sought to elicit participation from all mandatory enrollees and involved a significant proportion of individuals in job search. Unlike SWIM, the San Diego GAIN program provided basic

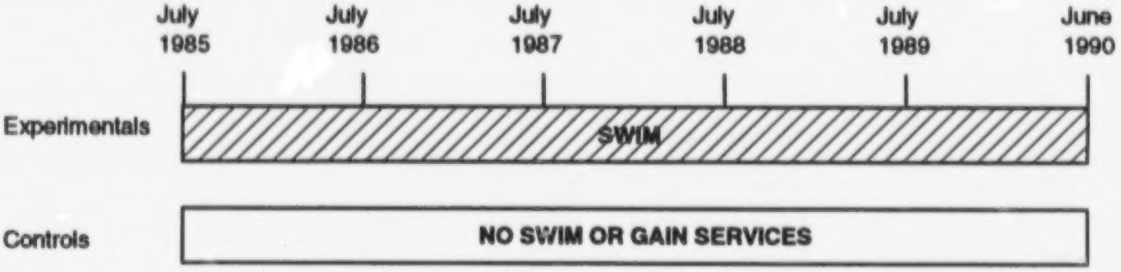
FIGURE 3.1

MEASURING SWIM IMPACTS:
ORIGINAL DESIGN AND FIVE-YEAR FOLLOW-UP OPTIONS

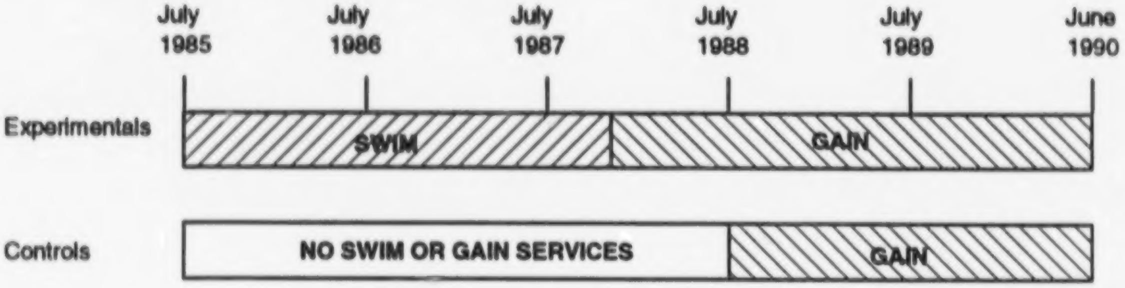
PANEL A: SWIM Demonstration Evaluation Design (Measures the Two-Year Impact of Providing Two Years of SWIM Treatment Followed by Nine Months of GAIN Treatment)


July 1985
July 1986
July 1987
July 1988
Experimentals
SWIM
GAIN
Controls
NO SWIM OR GAIN SERVICES

PANEL B: Preferred Design for Measuring the Five-Year Impact of SWIM as a Permanent Program


July 1985
July 1986
July 1987
July 1988
July 1989
June 1990
Experimentals
SWIM
Controls
NO SWIM OR GAIN SERVICES

PANEL C: Actual Extension of SWIM Demonstration Evaluation to Include Five Years of Follow-Up (Measures the Five-Year Impact of Providing Two Years of SWIM Followed by Three Years of GAIN, Compared to Three Years of No SWIM or GAIN Services Followed by Two Years of GAIN)


July 1985
July 1986
July 1987
July 1988
July 1989
June 1990
Experimentals
SWIM
GAIN
Controls
NO SWIM OR GAIN SERVICES
GAIN

education, usually as an initial component, to a large proportion of its participants, made little use of unpaid community work experience, and issued frequent temporary deferrals from program participation.[10]

In extending the SWIM follow-up beyond the two years originally envisioned, into the period during which San Diego operated GAIN, this supplemental five-year study thus extends the follow-up period without having extended the SWIM program for experimentals or the "service embargo" put in place for SWIM controls. This extension, depicted in Panel C of Figure 3.1, presents two problems. First, experimental group members were not subject to a permanent saturation program. For example, as shown in Panel C, experimentals who remained on AFDC or returned to the rolls in years three, four, and five did not experience key features of SWIM – its particular array and sequence of program components or its universal, ongoing participation requirement, possibly altering SWIM's potential effect on their behavior. Second, the control group was not excluded from mandatory welfare-to-work program services throughout the entire five-year follow-up period. As shown in Panel C, control group members were eligible to receive these services, through GAIN, in the latter part of the five-year follow-up period.

The inclusion of GAIN in the five-year follow-up period and the fact that both experimentals and controls were eligible for its services prevents a precise interpretation of measured impacts in the later years of the five-year SWIM follow-up period. If the five-year follow-up study is perceived as providing an indication of the possible impacts of a permanent SWIM program (as depicted in Panel B of Figure 3.1), the later-year impact estimates in this report must be viewed as underestimates of the impact of a permanent program. This is due to control group members' exposure to GAIN during the third, fourth, and fifth years of the follow-up period. The more GAIN participation by controls, the greater the underestimate. And, as discussed below, GAIN participation by controls was substantial. These results suggest that it is very probable that SWIM impacts past the two-year follow-up point would have been larger had controls not been assigned to GAIN's program components or subject to its participation mandate. This conclusion is supported by recently issued GAIN impact findings indicating that in San Diego County, GAIN increased earnings and reduced AFDC payments during the evaluation's first year of follow-up.[11]

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[10] For more details on San Diego GAIN implementation and participation patterns during this time period, see Freedman and Riccio (1991) and Riccio and Friedlander (1992).

[11] Riccio and Friedlander (1992) present first-year GAIN impact findings for six counties in California. In San Diego County, AFDC-FGs randomly assigned to an experimental group, which was required to enroll

(continued...)

If the five-year follow-up study is viewed as providing estimates of the longer-term effects of providing two years of SWIM's mandates and services (as depicted in Panel A of Figure 3.1), then the extent to which controls *and experimentals* participated in GAIN must be taken into account in order to assess the direction and magnitude of biases in the later-year impact estimates in this report.

To examine the potential importance of GAIN exposure and participation to interpreting the later years of the five-year SWIM follow-up period, a review of GAIN casefiles was conducted in San Diego for a random subsample of 401 SWIM experimentals and controls. The reviews sought to ascertain, for years two through five of the follow-up period, the extent to which SWIM controls and experimentals received GAIN services, the types of services received, and the timing of receipt of services. Results from these reviews are described in detail in Appendix A and shown in Appendix Tables A.1 through A.3.

To summarize, the discussion and data in Appendix A indicate that both experimentals and controls did, in fact, participate in GAIN activities to a significant degree in the latter part of the five-year follow-up period. GAIN participation rates among control group members were substantial: Approximately 20 percent of AFDC-FG and AFDC-U controls participated in GAIN during the five-year follow-up period. Among AFDC-FGs, the rate of GAIN participation was similar for experimentals and controls; among AFDC-Us, controls actually participated in GAIN somewhat more than experimentals.[12] In fact, the GAIN participation rate for AFDC-U controls was about 12

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[11](...continued)
in GAIN, had first-year follow-up earnings that were $345 higher than a randomly assigned control group, whose members were not eligible for GAIN. First-year follow-up AFDC payments were $302 lower for AFDC-FG experimentals than controls. Both of these impact estimates were statistically significant. Among AFDC-Us, first-year follow-up earnings were $241 higher for experimentals than controls (not statistically significant), and first-year follow-up AFDC payments were a statistically significant $510 lower for experimentals than controls.

[12] Data obtained through the GAIN casefile reviews were also used to determine the extent to which GAIN continued SWIM's mandate to involve a large share of those receiving AFDC in employment-directed activities on an ongoing basis, i.e., as long as they remained on the AFDC rolls. To examine this issue, monthly participation rates were calculated for the period of time covered by the five-year follow-up period, where the denominator of each rate consisted of those individuals in the 401-person sample who were receiving AFDC in that month and the numerator consisted of sample members receiving AFDC who were active in SWIM or GAIN activities during that month. The results indicate that, although GAIN did involve a significant proportion of individuals in program activities each month, monthly participation rates were somewhat higher in SWIM. Among experimentals, monthly participation rates averaged 36.7 percent in SWIM's first year and 23.1 percent in SWIM's second year. During the three months when SWIM was phasing down, prior to the start of GAIN, monthly participation rates averaged 14.5 percent. In the first nine months of GAIN operations, monthly participation rates averaged 7.3 percent. Following this initial start-up period, these rates
(continued...)

percentage points greater than the rate for AFDC-U experimentals. This difference, however, does not occur until years four and five of the follow-up period, indicating that the pattern of AFDC-U impacts described for the first three years of the follow-up period does not reflect possible effects of GAIN.

However, among both AFDC-FGs and AFDC-Us, GAIN represented the first exposure that control group members had to mandatory welfare-to-work programs in several years, while GAIN was the second "round" of welfare-to-work program exposure for experimentals. In fact, the casefile reviews indicated that GAIN secured participation from control group members who had previously not participated, on their own initiative, in employment-directed activities offered in the community during the years that SWIM operated. This was not the case for experimental group members: Almost all experimentals who participated in GAIN had previously participated in SWIM activities or had enrolled, on their own initiative, in community education or training activities prior to the start of GAIN. As shown in Appendix Table A.3, overall – combining SWIM, GAIN, and client-initiated activity for this casefile sample – 66 percent of the AFDC-FG experimentals and 42 percent of the AFDC-FG controls participated in activities intended to increase their employment during the five-year follow-up period, resulting in incremental participation totalling 24 percentage points. This is less than the 33-percentage-point incremental participation rate during the SWIM years for AFDC-FGs in this sample. Among the AFDC-Us, 75 percent of the experimentals and 33 percent of the controls participated in such activities during the full five-year follow-up period, resulting in incremental SWIM, GAIN, and self-initiated participation totalling 42 percentage points (see Appendix Table A.3). This is less than the 52-percentage-point incremental participation rate during the SWIM years for AFDC-Us in this sample.

In sum, the preceding discussion suggests that the third-, fourth-, and fifth-year impacts presented in this report surely underestimate the impact of SWIM as a permanent program. If SWIM had operated for the full five-year period, and if control group members had been prevented from receiving GAIN services throughout that period, it is likely that control group "catch-up" would

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[12] (...continued)
averaged 10.4 percent in the next 12-month period, 10.4 in the following 12-month period, and 7.7 percent in the last year for which data were available.

have been slower, resulting in a greater and longer-lasting experimental-control difference in earnings and AFDC receipt in the later follow-up years.[13]

The discussion also indicates that it is unclear whether the impacts past the two-year follow-up point are underestimates of the longer-term effects of providing two years of SWIM's mandates and services, although some evidence suggests that this may be the case. For the AFDC-Us, the later-year impact estimates presented in the report are likely to underestimate these impacts, since controls participated in GAIN at a higher rate than experimentals. For AFDC-FGs, the situation is more complicated. On the one hand, it is possible that the experiences of the AFDC-FG experimentals and controls in GAIN cancel each other out. On the other hand, controls' GAIN experiences, representing their first exposure to mandatory welfare-to-work programs in several years, may have influenced their behavior more than experimentals' exposure to GAIN, resulting in larger GAIN effects for controls, even though the two groups' GAIN participation rates were similar.[14]

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[13] GAIN's similarity to SWIM is one of the key reasons why control group participation in GAIN is an issue in interpreting the long-term SWIM results. Other events that could have affected controls and experimentals equally, such as changes in AFDC grant calculation methods in California, would not have raised significant interpretation issues regarding SWIM's impacts. GAIN, however, represents another version of the type of program whose effects the SWIM experimental design was intended to measure, and thus the exposure of controls to GAIN blurred the distinction between controls and experimentals in the later years of the follow-up period.

[14] Further work such as statistical simulations could be done to attempt to determine GAIN's effect on SWIM experimentals and controls, once three-year GAIN impact estimates are available Resources allocated for this study and the current availability of only one-year GAIN results did not permit such work.

CHAPTER 4

FIVE-YEAR IMPACTS ON EMPLOYMENT, EARNINGS, AND AFDC RECEIPT

This chapter presents five-year impacts on employment, earnings, and AFDC receipt. Results are discussed first for the AFDC-FG sample and then for the AFDC-U sample. The estimation approach is the same as that used in Hamilton and Friedlander (1989). Methodology and associated analysis issues are discussed in that report. The present analysis assumes that the reader is familiar with these issues. It is worth repeating, however, that impacts are estimated from an experimental design by comparing outcomes averaged over *all* experimentals to outcomes averaged over *all* controls. Among other things, this requires that averages for the experimental group include both SWIM program participants and those experimental sample members who did not participate. Similarly, dollar-denominated outcomes, such as average earnings and average AFDC payments, include zero amounts for sample members who were not employed or who were not on welfare during the time period covered.

Outcome data for employment, earnings, and AFDC receipt are organized in months (AFDC only), calendar quarters of three months, years of four quarters, and the full follow-up. Random assignment occurred in quarter one. Follow-up year one is defined as quarters two through five, year two consists of quarters six through nine, and so on. The five-year follow-up is quarters two through 21. Quarters, years, and full follow-up coincide exactly for earnings and AFDC. One extra quarter of AFDC data is available (quarter 22); this is shown in the quarter-by-quarter exhibits but is shown in full follow-up summary measures only in Appendices B and C.[1]

The outcome data discussed in this report cover the years 1985 through 1991. In the mid-1980s, the San Diego labor market was strong. Expansion was occurring at a fast pace, particularly in the service sector. Although a full evaluation of changes in the San Diego labor market, particularly the employment prospects for those eligible for SWIM, is beyond the scope of this report, it may be noted that unemployment rates worsened at the end of the SWIM evaluation's five-year follow-up period.[2]

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[1] The grouping of quarters into years in this report is different from the grouping in a forthcoming study by Daniel Friedlander and Gary Burtless. In that study, year one is defined as quarters one through four, year two as quarters five through eight, and so forth.

A. Impacts for the Single-Parent (AFDC-FG) Assistance Category

Over the five-year follow-up period, total earnings for AFDC-FG controls averaged $14,033 per sample member. For experimentals, this average was $16,109, for a gain or impact of $2,076, which is statistically significant and represents a 14.8 percent increase relative to the mean for controls. Most of this impact was associated with an increase in time employed rather than an increase in pay while working. The estimated five-year earnings impact for SWIM more than doubles the corresponding two-year impact estimate presented previously in Hamilton and Friedlander (1989). The pattern of experimental-control differences over time indicates, however, that the total earnings impact of SWIM will not continue to grow much after the five-year mark. Total earnings impacts for AFDC-FGs nevertheless remain large compared to five-year impacts available for other experimentally evaluated programs targeted to a large segment of the welfare caseload.[3]

Total AFDC payments over the five-year follow-up averaged $17,642 per control sample member. An average of $15,726 was observed for experimentals. The $1,916 reduction per experimental sample member was statistically significant and amounted to a 10.9 percent saving relative to the control mean. The bulk of the dollar reduction was associated with fewer months on AFDC rather than lower monthly grant amounts for those who remained on public assistance. Five-year savings were twice those found previously for the two-year follow-up. As with the earnings impact, the total for AFDC reductions is not expected to grow much more over time. Nonetheless, savings for AFDC-FGs were the largest estimated experimentally for a broad-coverage program over a five-year period.

1. Employment and Earnings. Table 4.1 presents impact estimates for the AFDC-FG sample. The table first shows employment, then earnings, and then AFDC receipt and AFDC payments. Summary measures are shown near the top of each panel; estimates for each of the five follow-up

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[2] According to statistics maintained by the California Employment Development Department, annual unemployment rates from 1985 through 1987 for the County of San Diego were 5.3, 5.0, and 4.5; statistics kept by the Bureau of Labor Statistics indicate that annual unemployment rates from 1988 through 1991 for the City of San Diego were 4.3, 4.0, 4.4, and 6.2.

[3] Few studies of welfare employment programs have had sufficient data to calculate five-year impacts. Among these few, only the Baltimore Options program produced five-year earnings impacts of similar magnitude to those of San Diego SWIM. See Friedlander and Burtless (forthcoming) for those results.

TABLE 4.1

SWIM

ALL AFDC-FG: IMPACTS ON EMPLOYMENT, EARNINGS, AFDC RECEIPT, AND AFDC PAYMENTS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Ever employed (%)				
Quarters 2-21	74.6	67.5	7.1 ***	10.5%
Average quarterly employment rate (%)				
Quarters 2-21	33.9	29.0	4.8 ***	16.7%
Quarters 2-5	33.0	25.7	7.3 ***	28.4%
Quarters 6-9	35.2	27.9	7.2 ***	25.9%
Quarters 10-13	34.4	28.2	6.2 ***	22.0%
Quarters 14-17	33.5	31.3	2.2	7.0%
Quarters 18-21	33.3	32.0	1.3	4.0%
Average total earnings ($)				
Quarters 2-21	16109	14033	2076 **	14.8%
Quarters 2-5	2029	1678	352 ***	21.0%
Quarters 6-9	2892	2248	644 ***	28.6%
Quarters 10-13	3287	2732	555 ***	20.3%
Quarters 14-17	3775	3397	378	11.1%
Quarters 18-21	4126	3978	148	3.7%
Average earnings per quarter employed ($)				
Quarters 2-21	*2378*	*2417*	*-39 (a)*	*-1.6%*
Quarters 2-5	*1535*	*1629*	*-94 (a)*	*-5.8%*
Quarters 6-9	*2056*	*2012*	*44 (a)*	*2.2%*
Quarters 10-13	*2391*	*2424*	*-33 (a)*	*-1.4%*
Quarters 14-17	*2816*	*2712*	*105 (a)*	*3.9%*
Quarters 18-21	*3100*	*3108*	*-9 (a)*	*-0.3%*

(continued)

Table 4.1 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Ever received any AFDC payments (%)				
Quarters 2-21	92.9	93.2	-0.3	-0.3%
Average number of months receiving AFDC payments				
Quarters 2-21	28.46	31.31	-2.85 ***	-9.1%
Quarters 2-5	8.60	9.13	-0.53 ***	-5.9%
Quarters 6-9	6.34	7.23	-0.89 ***	-12.3%
Quarters 10-13	5.22	5.95	-0.73 ***	-12.2%
Quarters 14-17	4.41	4.87	-0.45 **	-9.3%
Quarters 18-21	3.89	4.13	-0.25	-6.0%
Average total AFDC payments received ($)				
Quarters 2-21	15726	17642	-1916 ***	-10.9%
Quarters 2-5	4419	4838	-419 ***	-8.7%
Quarters 6-9	3407	3968	-560 ***	-14.1%
Quarters 10-13	2952	3435	-483 ***	-14.1%
Quarters 14-17	2621	2905	-284 **	-9.8%
Quarters 18-21	2327	2496	-169	-6.8%
Average AFDC payment per month received ($)				
Quarters 2-21	*553*	*563*	*-11 (a)*	*-1.9%*
Quarters 2-5	*514*	*530*	*-16 (a)*	*-3.0%*
Quarters 6-9	*538*	*549*	*-11 (a)*	*-2.0%*
Quarters 10-13	*565*	*577*	*-12 (a)*	*-2.1%*
Quarters 14-17	*594*	*597*	*-3 (a)*	*-0.6%*
Quarters 18-21	*599*	*604*	*-5 (a)*	*-0.8%*
Sample size (total = 3210)	1604	1606		

(continued)

Table 4.1 (continued).

SOURCE: MDRC calculations from the County of San Diego AFDC records and the State of California Unemployment Insurance earnings records.

NOTES: The sample for this table consists of individuals who registered between July 1985 and June 1986.

Unless shown in italics, dollar averages include zero values for sample members not employed and for sample members not receiving welfare. Estimates are regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members. Rounding may cause slight discrepancies in calculating sums and differences.

Italicized estimates cover only non-zero earnings amounts or non-zero AFDC amounts. Differences between experimentals and controls for such "conditional" estimates are not true experimental comparisons.

"Percent difference" equals 100 times "difference" divided by "controls."

For all measures, the quarter of random assignment refers to the calendar quarter in which random assignment occurred. Because quarter 1, the quarter of random assignment, may contain some earnings and AFDC payments from the period prior to random assignment, it is excluded from the summary measures of follow-up.

A two-tailed t-test was applied to differences between experimental and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

(a) Not an experimental comparison; statistical tests not performed.

years are shown below. From left to right, the table shows regression-adjusted means for the experimental group and then for the control group; the difference between means, which is the impact estimate, along with an indication of the statistical significance of the difference; and the difference stated as a percent of the control mean. Rows appearing in italics contain supplemental estimates that do not represent true experimental impacts, and statistical tests are not shown for these. The accompanying Figure 4.1 displays graphically over the follow-up period the quarter-by-quarter impact estimates for earnings and AFDC payments. In order to assist the visual comparison of these two curves, the sign of impacts on AFDC payments is reversed, translating negative "reductions" into positive "savings." Figure 4.2 shows quarterly employment rates for experimentals and controls over the same follow-up period. Tables and figures are presented with additional detail, including the quarter-by-quarter estimates, in Appendix B.

We look first at the annual estimates for employment and earnings impacts. Earnings impacts exhibit a pattern of increase from year one to year two, followed by some decline in years three, four, and five. The maximum annual earnings impact is $644 for year two (statistically significant), but the effect declines to $148 in year five (not statistically significant). Employment impacts follow the same pattern but may peak somewhat earlier than earnings impacts. The pattern of impacts over time is shown in finer detail in the quarter-by-quarter earnings impact estimates of Figure 4.1. The graph shows a peak in earnings impact around quarter six and suggests that not much will remain of the earnings impact after year five.

Table 4.1 also shows earnings per quarter of employment (i.e., average earnings using only quarters with earnings), with separate averages for experimentals and controls. These amounts are calculated by dividing regression-adjusted mean earnings over a specified period by the regression-adjusted mean number of quarters of employment over the same period (not shown in the table).[4] These "conditional" earnings averages are not much different for experimentals and controls, either for the follow-up period as a whole or for particular follow-up years. Over the full follow-up period, earnings per employed quarter were only $39 or 1.6 percent less for experimentals than for controls. There is no consistent pattern of positive or negative differences over time. Thus, differences in earnings on the job do not account for much of the earnings impact of SWIM. Rather, it is the

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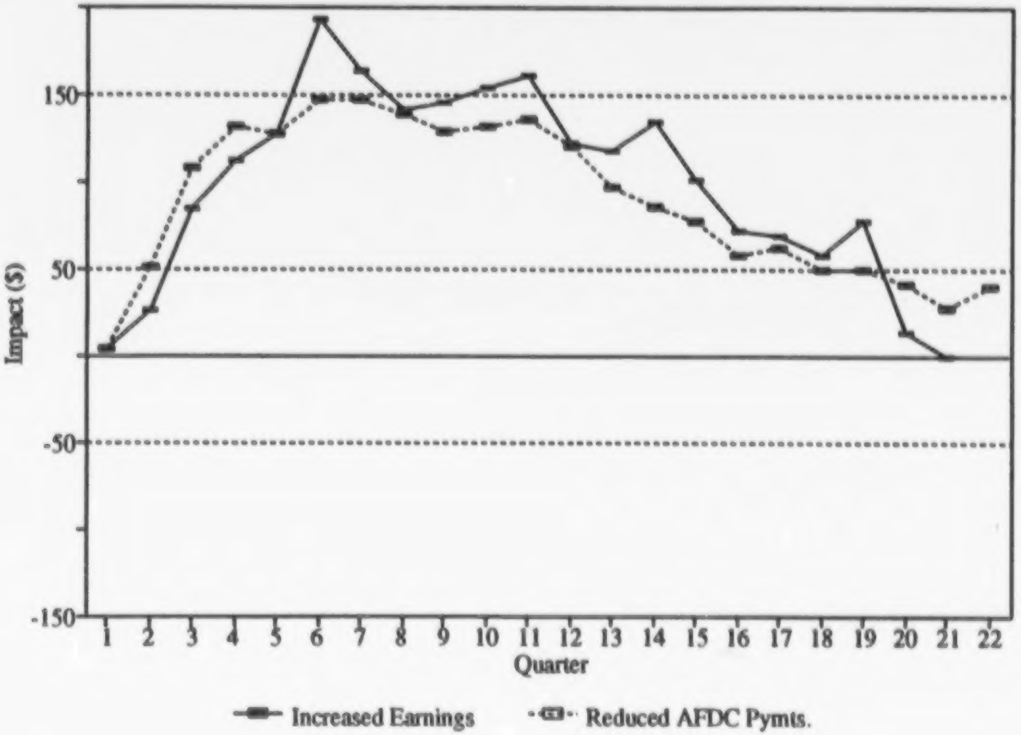
[4] The number of quarters of employment in a period may be calculated from the average quarterly employment rate in the same period by dividing the latter by 100 and multiplying by the number of quarters in the period, which would be four quarters for an annual estimate and 20 quarters for the full sample estimate.

FIGURE 4.1

SWIM

ALL AFDC-FG: QUARTERLY IMPACTS ON EARNINGS AND AFDC PAYMENTS

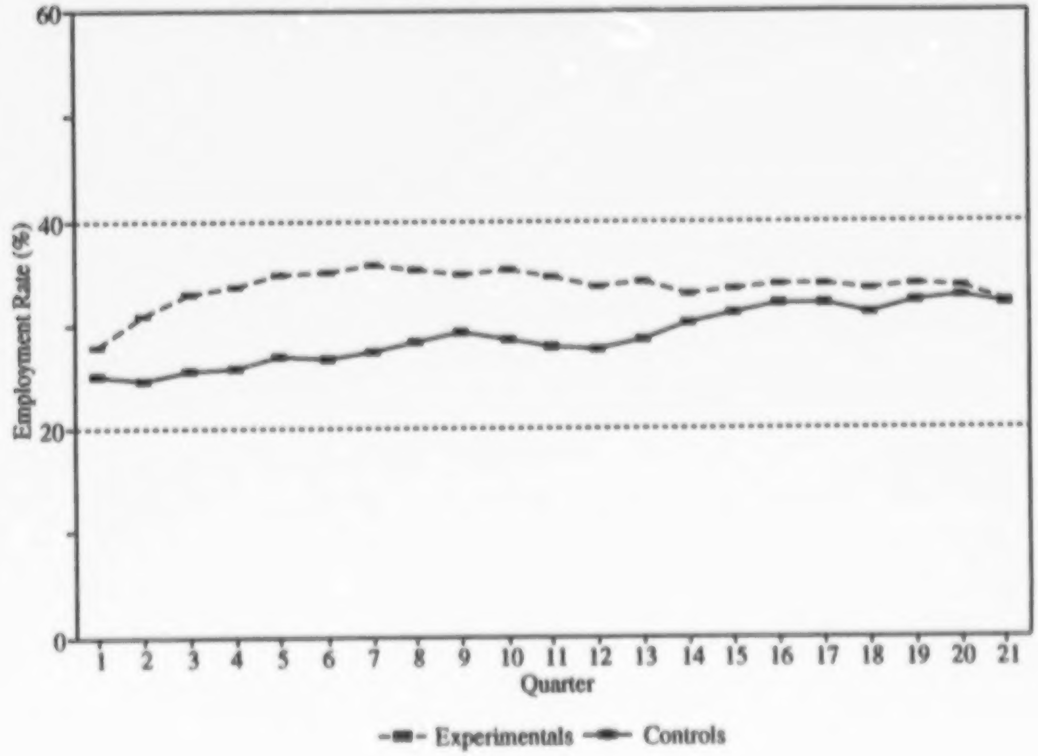


SOURCE: Appendix Table B.1.

FIGURE 4.2

SWIM

ALL AFDC-FG: QUARTERLY EMPLOYMENT RATES



SOURCE: Appendix Table B.1.

increase in employment that accounts for the increase in earnings. This result also implies that jobs obtained by experimentals were, on average, similar to jobs obtained by controls.[5]

Further evidence of the nature of the effects of SWIM over time may be seen in Figure 4.2. The solid line in this figure shows the quarterly employment rates of controls. These rates rise gradually over time, from quarter two (the quarter following the quarter of random assignment) through the end of the follow-up period. This increase in employment represents the sum of two factors: (1) the natural propensity of individuals on welfare to find jobs eventually, plus (2) possible effects of control group participation in local training activities or, during the later years of follow-up, in GAIN. The increase in control group employment gives SWIM a higher and higher target that it must surpass in order to have long-term employment and earnings impacts. Stated a bit differently, the increase in control group employment over time may make controls appear to "catch up" to employment rates of experimentals, either completely or partially, thus cutting into the long-term experimental-control differential. Control group catch-up is one explanation for the narrowing of the impact differential over time. Catch-up is independent of the direct effects of SWIM on its enrollees and does not imply that SWIM services were ineffective.

The quarter-by-quarter employment rates of experimentals show a different pattern. Instead of gradual and steady growth, the experimental curve shows a rapid increase, up to a peak of nearly 36 percent around quarter seven, followed by a decline to around 33 percent by year four, possibly holding steady after than point.[6] (See also Appendix Table B.1.) This decline suggests that some of the effect of SWIM "wore off" over time, leading to job loss followed by joblessness. It may be that SWIM speeded up the rate at which experimentals started work, but that some of the jobs they found were largely the high turnover jobs typically found by this population. Under this hypothesis, jobs started sooner but did not last any longer. The peak in experimental group employment may therefore reflect a bunching up of employment near the period of exposure of experimentals to the SWIM demonstration.[7] One implication would be that the employment and earnings advantage of SWIM enrollees over controls could perhaps be made "permanent," without a decline over time, by making the SWIM program itself permanent so that services could be delivered and the participation mandate applied again to former enrollees who lose jobs and return to AFDC.

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[5] The comparison of conditional earnings of experimentals and controls is not a true experimental comparison because some sample members (the zero earners) have been dropped from the comparison. It is therefore difficult to infer causality in such a comparison. Although it is true that jobs of experimentals were similar (at least in quarterly pay) to jobs of controls *on average*, this result may have come about because some experimentals got much higher-paying jobs than controls, while others got much lower-paying jobs. (Some experimentals may have taken low-wage jobs to avoid the SWIM participation mandate; others may have been urged to take these jobs as "stepping stones" to higher-paying jobs.) The inference that SWIM did not affect earnings per quarter of employment appears more likely than the situation in which some experimentals obtained much higher-paying jobs than controls while others got much lower-paying jobs.

[6] Earnings in quarter one may have included some earnings that preceded a person's random assignment and thus are irrelevant to the impacts.

One important question bearing on this discussion is whether SWIM acted to speed up employment by experimentals who would have found work anyway or, in addition, helped individuals who would not have worked to find jobs. In Table 4.1, the variable "ever employed, quarters 2-21" can help answer this question. If SWIM only speeded up employment for those who would have worked anyway, then there should be no experimental-control difference for this outcome. In fact, there is a 7.1 percentage point difference (statistically significant), a substantial impact judged against other experiments. The magnitude of this impact can best be judged against the mean of the outcome for the control group. This mean is 67.5 percent, which, when subtracted from 100, implies that 32.5 percent of all controls never produced earnings (under the Unemployment Insurance system) during the five years of follow-up. This 32.5 percent rate of long-term joblessness was reduced by 7.1 percentage points. In other words, about one in every five SWIM experimentals who would not have worked in the absence of the program did so at some time as a result of the program. Thus, SWIM did more than speed up the start of jobs: It also decreased the number of enrollees who did not work at all over the five years.

Table 4.2 presents more data on the earnings levels of jobs obtained by experimentals and controls. The top panel of the table shows the percentage of experimentals and controls in several annual earnings brackets for year two and year five. The differences between these percentage distributions constitute estimates of SWIM program impacts. For year two, it may be seen that SWIM reduced by 9.2 percentage points the number of experimental sample members who had no earnings during the year. At the same time, there was a 2.6 percentage point increase in the number of experimentals earning less than $2,000 for the year, probably because they worked for only a short time or part time. There were similar increases in the next two brackets, $2,000-$4,999 and $5,000-$9,999. At $10,000 and above, there was only a 1.6 percentage point increase.

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[7] See Friedlander and Burtless (forthcoming) for further discussion of this and other hypotheses concerning the nature of the impacts of SWIM and three other welfare-to-work programs.

TABLE 4.2

SWIM

ALL AFDC-FG: IMPACTS ON THE DISTRIBUTION OF EARNINGS DURING THE SECOND AND FIFTH YEAR OF FOLLOW-UP

	Quarters 6 - 9			Quarters 18 - 21		
Outcome	Experimentals	Controls	Difference	Experimentals	Controls	Difference
Average annual earnings (%)						
None	50.6	59.8	-9.2 ***	56.7	58.3	-1.6
$1 - $1,999	17.0	14.4	2.6 **	9.8	9.7	0.2
$2,000 - $4,999	10.8	8.8	2.0 *	7.6	7.0	0.6
$5,000 - $9,999	12.2	9.1	3.0 ***	9.7	8.9	0.8
$10,000 or more	9.4	7.8	1.6 *	16.2	16.1	0.1
Total	100.0	100.0	0.0	100.0	100.0	0.0
Average annual earnings, if employed (%) (b)						
$1 - $1,999	*34.5*	*35.9*	*-1.4 (a)*	*22.7*	*23.2*	*-0.5 (a)*
$2,000 - $4,999	*21.8*	*21.9*	*-0.1 (a)*	*17.5*	*16.8*	*0.7 (a)*
$5,000 - $9,999	*24.6*	*22.7*	*1.9 (a)*	*22.4*	*21.3*	*1.1 (a)*
$10,000 or more	*19.1*	*19.5*	*-0.4 (a)*	*37.4*	*38.7*	*-1.3 (a)*
Total	*100.0*	*100.0*	*0.0*	*100.0*	*100.0*	*0.0*
Sample size (total = 3210)	1604	1606		1604	1606	

SOURCES AND NOTES: See Table 4.1.

(a) Not an experimental comparison; statistical tests not performed.

(b) Estimates in this panel are based only on persons with earnings. Statistical tests were not applied to the differences.

The lower panel converts these same estimates into conditional form by dividing the bracketed percentages by the share of the experimentals or controls who worked during the year. Thus, for example, the 17.0 percent of all experimentals who earned $1-$1,999 represent 34.5 percent of experimentals who had any earnings during the year. The differences in the lower panel are not true experimental impacts, since sample members without earnings are dropped. But the small conditional differences nevertheless indicate that the increase in employment brought about by SWIM in year two led to a similar distribution of earnings between employed experimentals and employed controls. That is, the earnings levels of jobs did not shift up or down as a result of SWIM.

The right side of Table 4.2 shows the same distributions for year five. It is clear that little difference remains between experimentals and controls by this time. It may also be noted that the distribution of earnings has, in general, shifted up over time for both experimentals and controls. As shown in the lower panel, 35.9 percent of employed controls in year two earned in the lowest bracket, a number which shrunk to 23.2 percent by year five. At the same time, only 19.5 percent were earning $10,000 or more in year two, compared to 38.7 percent (almost twice the number) in year five. The same shift upward occurred for experimentals. This upward shift indicates earnings increases associated with greater stability of job-holding, longer hours, higher hourly wage rates, or a combination of these factors. These increases apparently occurred independent of any SWIM program effect.

2. **AFDC Receipt and AFDC Payments**. Table 4.1 and Figure 4.1 also show the pattern of AFDC impacts over time. In Table 4.1, the annual impact estimates for average number of months receiving AFDC and average AFDC payments both show an increase from the first year to the second, with a peak reduction in the year two of 0.89 months and $560. Both were statistically significant, and the latter represents a 14.1 percent saving relative to AFDC payments for controls in that year. By year five, the experimental-control difference in number of months receiving AFDC was down to 0.25, and the saving was down to $169, although this was still 6.8 percent of the control mean. The same pattern of impacts over time is revealed in Figure 4.1 in quarter-by-quarter detail. In this graph, the peak in dollar savings shows up at around quarter six. The graph also suggests that some of the experimental-control differential in AFDC payments may continue beyond year five. In addition, it may be observed how closely the quarterly patterns of impacts on earnings and AFDC payments match each other in this figure.

AFDC payments per month received are also shown (in italics) in Table 4.1. As with conditional earnings, these amounts are calculated by dividing regression-adjusted average AFDC payments over a time period by the regression-adjusted number of months receiving AFDC during the same time period. The differences in these conditional amounts for experimentals and controls are small, indicating that the great bulk of the overall impact on AFDC payments came from fewer months of receipt rather than lower monthly payments for experimentals who remained on AFDC. Over the full follow-up period, AFDC payments per month received were only $11 per month or 1.9 percent less for experimentals than for controls. The largest conditional difference was in year one, amounting to $16 per month or 3.0 percent.

An important question for SWIM is whether a significant share of the welfare reductions were associated with a *deterrence effect*, which would be associated with the relative cost hypothesis. Conceptually, such effects mean that some individuals who would otherwise remain on AFDC are deterred from doing so because SWIM has increased the difficulty, effort, or stigma of maintaining an AFDC grant. Empirically, deterrence may be observed as an increase in the number of individuals who leave AFDC without employment.[8] Table 4.3 presents a breakdown of employment and AFDC receipt status that may help identify possible deterrence and related effects.

For each quarter of follow-up, a sample member is categorized as (1) not employed and received AFDC in at least one of the three months, (2) employed and received AFDC in at least one month, (3) employed and did not receive any AFDC, and (4) not employed and did not receive any AFDC. For each year and for the follow-up period as a whole, the percentage of all quarters spent in each status is shown for experimentals and controls, as is the difference. Details of quarter-by-quarter estimates are shown in Appendix Table B.2.

The impacts of SWIM brought about a clear reduction in the percentage of time not employed and on AFDC, with a peak effect in year two. A corresponding effect was the increase in employment and AFDC receipt in the same quarter. This effect was concentrated in year one and the first half of year two, and simply manifests the initial transition occurring for experimentals who obtain jobs and leave AFDC in the same quarter. The larger employment effect is the increase in quarters of employment without any AFDC receipt.

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[8] However, an increase in this status could also be the result of sample members becoming married, a common route off the AFDC rolls (see Ellwood, 1986).

TABLE 4.3

SWIM

ALL AFDC-FG: IMPACTS ON COMBINED EMPLOYMENT AND AFDC RECEIPT STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference
Not employed, received AFDC (%)			
Average quarterly rate			
Quarters 2-21	37.3	43.4	-6.1 ***
Quarters 2-5	54.2	62.7	-8.6 ***
Quarters 6-9	40.8	50.6	-9.8 ***
Quarters 10-13	34.5	42.2	-7.8 ***
Quarters 14-17	30.2	33.0	-2.8 *
Quarters 18-21	26.9	28.7	-1.8
Employed, received AFDC (%)			
Average quarterly rate			
Quarters 2-21	12.9	11.3	1.6 ***
Quarters 2-5	22.1	17.3	4.8 ***
Quarters 6-9	15.0	12.6	2.5 ***
Quarters 10-13	11.6	9.6	1.9 **
Quarters 14-17	8.6	9.5	-0.9
Quarters 18-21	7.1	7.4	-0.3
Employed, did not receive AFDC (%)			
Average quarterly rate			
Quarters 2-21	21.0	17.7	3.2 ***
Quarters 2-5	10.9	8.4	2.5 ***
Quarters 6-9	20.1	15.4	4.8 ***
Quarters 10-13	22.8	18.5	4.3 ***
Quarters 14-17	24.9	21.8	3.1 **
Quarters 18-21	26.1	24.6	1.6
Not employed, did not receive AFDC (%)			
Average quarterly rate			
Quarters 2-21	28.8	27.5	1.3
Quarters 2-5	12.8	11.5	1.2
Quarters 6-9	24.1	21.5	2.6 **
Quarters 10-13	31.2	29.6	1.6
Quarters 14-17	36.3	35.7	0.6
Quarters 18-21	39.8	39.3	0.5
Sample size (total = 3210)	1604	1606	

SOURCES AND NOTES: See Table 4.1.

The final panel of the table evidences a possible small deterrence effect. This is an increase, primarily in year two, in the number of quarters with neither earnings nor AFDC receipt. It is not clear whether this effect is primarily associated with individuals leaving AFDC in response to the SWIM participation requirement, or with some lag in the return to AFDC of former SWIM enrollees who started to work and then terminated employment. In either case, the effect contributed to the overall AFDC savings of the SWIM program and to the high ratio of dollar AFDC reductions to earnings gains. However, the effect on "no work and no welfare" status does not persist much beyond follow-up year two.

Did SWIM have an impact on long-term AFDC receipt? This question may be interpreted in two ways. First, it may be asked whether SWIM reduced the rate of AFDC receipt in the long run. After five years, the rates of AFDC receipt for experimentals and controls are converging, so the answer to this question would appear to be negative. As discussed above, however, it is possible that making the SWIM program permanent might have yielded permanent reductions in AFDC because the services and mandate would apply to former enrollees returning to AFDC after a spell off.

An alternative form of the long-term receipt question is whether SWIM had any impact on individuals who would have remained on AFDC for five years with an uninterrupted AFDC spell in the absence of an intervention. This is difficult to answer from the estimates presented. If SWIM did affect long-term "stayers," however, it would likely only have been to interrupt their long spell with some time off the rolls. Had they achieved long-term independence from AFDC, then the long-term rate of AFDC receipt (i.e., the percentage receiving AFDC at the end of five years) would most likely have been reduced.

B. Impacts for the Two-Parent (AFDC-U) Assistance Category

Table 4.4 presents the five-year impact estimates for the AFDC-U sample in SWIM. The sample of AFDC-Us is smaller than the AFDC-FG sample. Hence, impacts of a similar magnitude are less likely to be statistically significant for AFDC-Us, particularly for earnings.

Note should also be taken of the method of tracking AFDC payments for AFDC-U cases. In the SWIM demonstration, AFDC payments for AFDC-Us were tracked using the AFDC case number of the AFDC-U sample member as of random assignment. If the AFDC-U sample member (the usually male head of a two-parent household) left the household, the family could become an AFDC-FG case. The evaluation did, however, continue to track AFDC payments going to that new AFDC-FG case, which retained the original case number. This method of data collection ensured

TABLE 4.4

SWIM

ALL AFDC-U: IMPACTS ON EMPLOYMENT, EARNINGS, AFDC RECEIPT, AND AFDC PAYMENTS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Ever employed (%)				
Quarters 2-21	79.3	75.4	4.0 *	5.2%
Average quarterly employment rate (%)				
Quarters 2-21	39.4	35.7	3.7 **	10.4%
Quarters 2-5	37.5	32.0	5.6 ***	17.5%
Quarters 6-9	41.8	36.9	4.8 **	13.1%
Quarters 10-13	40.1	36.3	3.8 *	10.4%
Quarters 14-17	40.2	37.6	2.6	7.0%
Quarters 18-21	37.3	35.6	1.8	4.9%
Average total earnings ($)				
Quarters 2-21	22878	21818	1060	4.9%
Quarters 2-5	3303	2815	487 *	17.3%
Quarters 6-9	4308	3831	478	12.5%
Quarters 10-13	4797	4448	350	7.9%
Quarters 14-17	5211	5214	-4	-0.1%
Quarters 18-21	5259	5510	-251	-4.6%
Average earnings per quarter employed ($)				
Quarters 2-21	*2905*	*3059*	*-154 (a)*	*-5.0%*
Quarters 2-5	*2200*	*2202*	*-3 (a)*	*-0.1%*
Quarters 6-9	*2578*	*2592*	*-14 (a)*	*-0.5%*
Quarters 10-13	*2994*	*3063*	*-69 (a)*	*-2.3%*
Quarters 14-17	*3242*	*3471*	*-229 (a)*	*-6.6%*
Quarters 18-21	*3524*	*3874*	*-350 (a)*	*-9.0%*

(continued)

Table 4.4 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Ever received any AFDC payments (%)				
Quarters 2-21	90.0	90.2	-0.2	-0.2%
Average number of months receiving AFDC payments				
Quarters 2-21	27.79	29.14	-1.34	-4.6%
Quarters 2-5	7.59	7.95	-0.35	-4.4%
Quarters 6-9	5.86	6.31	-0.44	-7.0%
Quarters 10-13	5.12	5.46	-0.34	-6.2%
Quarters 14-17	4.68	4.88	-0.20	-4.2%
Quarters 18-21	4.54	4.54	-0.00	-0.1%
Average total AFDC payments received ($)				
Quarters 2-21	19093	21054	-1961 **	-9.3%
Quarters 2-5	4888	5303	-415 **	-7.8%
Quarters 6-9	3896	4455	-558 ***	-12.5%
Quarters 10-13	3558	4036	-479 **	-11.9%
Quarters 14-17	3406	3730	-324	-8.7%
Quarters 18-21	3345	3530	-185	-5.2%
Average AFDC payment per month received ($)				
Quarters 2-21	*687*	*723*	*-36 (a)*	*-4.9%*
Quarters 2-5	*644*	*667*	*-24 (a)*	*-3.6%*
Quarters 6-9	*664*	*706*	*-42 (a)*	*-5.9%*
Quarters 10-13	*695*	*739*	*-44 (a)*	*-6.0%*
Quarters 14-17	*728*	*764*	*-36 (a)*	*-4.7%*
Quarters 18-21	*737*	*777*	*-40 (a)*	*-5.1%*
Sample size (total = 1340)	686	654		

SOURCES AND NOTES: See Table 4.1.

(a)Not an experimental comparison; statistical tests not performed.

that the evaluation would capture any effects of SWIM on AFDC payments, whether from reduced monthly grant payments, case closure, or the reduction in family size occurring from changes in family composition.

Total earnings for AFDC-U controls over the five-year follow-up period averaged $21,818 per sample member. This average increased to $22,878 for experimentals. The difference of $1,060, or 4.9 percent above the control mean, was not statistically significant. As for AFDC-FGs, most of this gain was associated with an increase in time employed rather than an increase in pay while working. The estimated five-year earnings impact for AFDC-Us in SWIM is only 11 percent greater than the two-year estimate of Hamilton and Friedlander (1989). The pattern of experimental-control differences over time reveals a rather rapid fall-off during year three – more rapid than for AFDC-FGs – with not much added to total earnings gains beyond that point. Rapid decline of earnings impacts for AFDC-Us is consistent with results for AFDC-Us in an earlier experimental evaluation of job search and unpaid work experience in San Diego.[9]

Over the five-year follow-up, AFDC payments averaged $21,054 per AFDC-U control sample member and $19,093 for experimentals. The $1,961 difference was statistically significant and constituted a 9.3 percent reduction relative to the control mean. Total AFDC savings were nearly twice the earnings gains measured over the same follow-up period. Unlike the impact for AFDC-FGs, less than half the savings for AFDC-Us was associated with fewer months on welfare. Lower monthly grant amounts for persons remaining on aid appeared to play a greater role for AFDC-Us than for AFDC-FGs, although the reason is not clear. Five-year AFDC savings were twice the two-year savings reported previously. The narrowing of the experimental-control difference over time was less rapid for AFDC payments than for earnings and was quite similar to the time shape of the same measure for AFDC-FGs. As is the case for AFDC-FGs, total savings are not expected to continue to grow much with additional follow-up beyond the five-year mark. The total amount of savings is as large as it was for AFDC-FGs, although the dearth of evaluation research for AFDC-Us limits comparisons with other programs.

1. **Employment and Earnings**. Table 4.4 and Figures 4.3 and 4.4 present impact estimates for the AFDC-U sample, using the same format as for AFDC-FGs. Appendix C gives the quarter-by-

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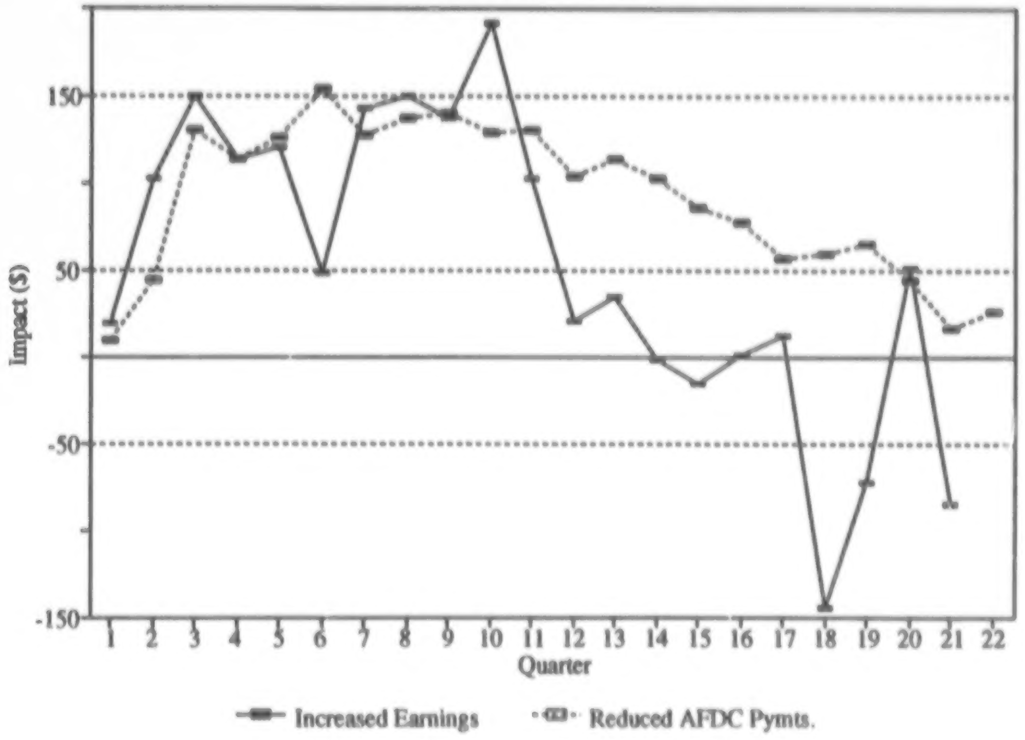
[9] The three-year Employment Preparation Program/Experimental Work Experience Program (EPP/EWEP) evaluation in the early 1980s yielded earnings impacts for AFDC-Us that did not last beyond year one. See Goldman, Friedlander, and Long (1986) for the full set of impact estimates from that evaluation.

FIGURE 4.3

SWIM

ALL AFDC-U: QUARTERLY IMPACTS ON EARNINGS AND AFDC PAYMENTS

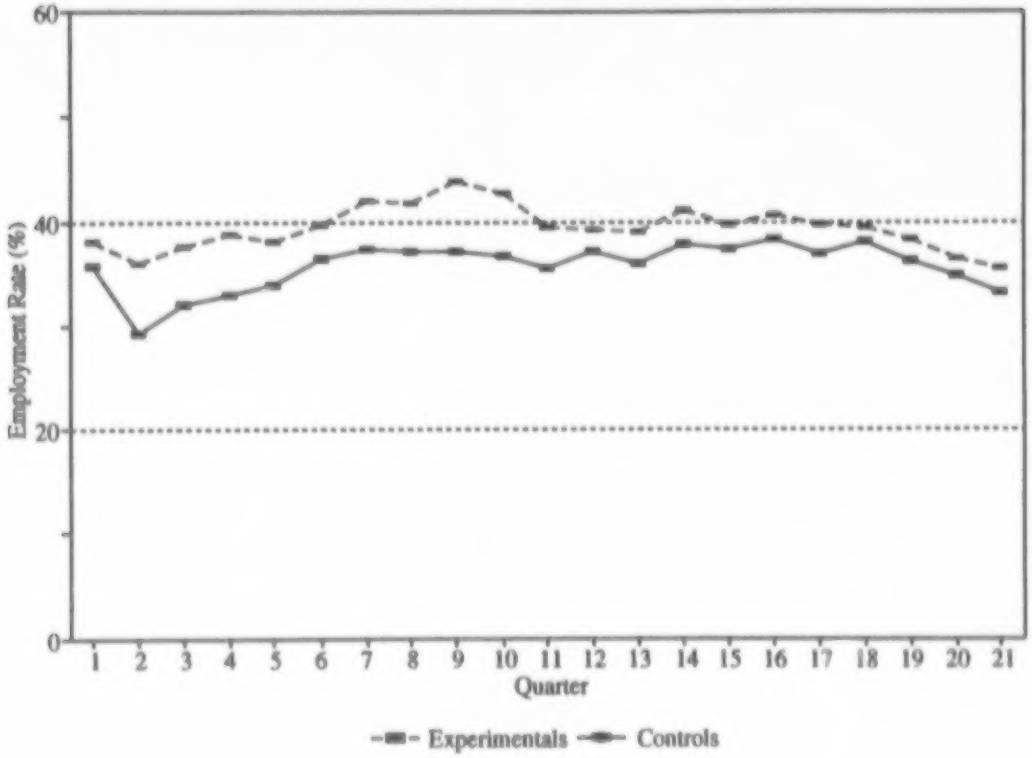


SOURCE: Appendix Table C.1.

FIGURE 4.4

SWIM

ALL AFDC-U: QUARTERLY EMPLOYMENT RATES



SOURCE: Appendix Table C.1.

quarter details. The annual estimates for employment and earnings impacts both evidence a maximum in year one and a leveling off in year two, with a sharp decline after year three. The maximum annual earnings impact is $487, statistically significant, for year one. By year five, a negative difference is estimated, although this is not statistically significant. Figure 4.3 shows a steep fall-off in earnings impacts from a peak in quarter 10 to nearly zero within two quarters. (See also Appendix Table C.1.) It is clear from the graph that no additions to the total earnings impact should be expected from further follow-up data.

The italicized estimates of earnings per quarter of employment indicate that differences in pay while working may have contributed to the overall pattern of impacts for AFDC-Us. These conditional average earnings were similar for experimentals and controls during follow-up year one, but the means for controls climbed more quickly than for experimentals. By year five, the negative difference amounted to 9.0 percent of the control mean. Thus, although the amount of employment among AFDC-U experimentals and controls was similar in year five, the former group earned less at work, which contributed to the overall negative earnings difference between experimentals and controls in that year. Because this impact is not statistically significant, there is some uncertainty that the true impact is actually negative; but it is clear that SWIM did not *improve* the quality of jobs held by AFDC-U experimentals, at least as far as on-the-job earnings reflect quality.

The quarterly employment rates for experimentals and controls, shown in Figure 4.4, reveal more about the nature of SWIM impacts on AFDC-Us. The solid line for controls shows a pronounced dip at the start of follow-up, but with a much quicker rise and leveling off than for AFDC-FGs. By the beginning of follow-up year two, controls have reached an employment peak, and a plateau extends from that point onward. The initial impact for AFDC-U enrollees appears to be in avoiding the employment dip in quarter two. Experimentals maintain a lead over controls through the second year, but, instead of leveling off, their employment rate drops after that point. This falling off of employment from a peak was also seen for AFDC-FG experimentals. The overall pattern suggests that control group "catch-up," as well as some "wearing-off" of the SWIM effect, was responsible for the narrowing of the experimental-control differential over time. As discussed above for AFDC-FGs, it is not clear whether a permanent SWIM program would have prevented the wearing-off phenomenon and led to a permanent experimental-control differential in employment and earnings.

Did SWIM for AFDC-Us achieve impacts by speeding up employment? The variable "ever employed, quarters 2-21" shows a smaller experimental-control difference, only 4.0 percentage points, than for AFDC-FGs. But the control mean for this measure is greater for AFDC-Us than for AFDC-FGs. This higher mean gives SWIM a higher target to beat. But it also means that fewer controls, only 24.6 percent, did not have Unemployment Insurance reported earnings during the five-year follow-up. The reduction of this jobless rate by 4.0 percentage points implies that one in six AFDC-U enrollees who would not have worked did obtain at least some earnings under SWIM. This ratio is smaller than for AFDC-FGs, but not much smaller. Thus, speeding up employment was only part of the full effect of SWIM.

The percentage distribution of AFDC-Us across earnings bracket is shown in Table 4.5. For both year two and year five, the largest increase in employment came in the lowest earnings bracket, $1-$1,999. In fact, as the lower panel shows, the distribution of earnings for employed AFDC-Us shifted up faster between year two and year five for controls than for experimentals. This result corresponds to the negative difference in earnings per quarter of employment noted above.

2. AFDC Receipt and AFDC Payments. As shown in Table 4.4 and Figure 4.3, AFDC impacts for AFDC-Us increased from year one to year two and then declined. Time on aid in year two was reduced by 0.44 months per enrollee, which is not statistically significant. AFDC payments in year two were down $558 per enrollee, a statistically significant impact, 12.5 percent of average payments to controls. The impact on months reached zero in year five, and the payments impact was down to $185, although this amount was still 5.2 percent of the control mean for the year. In Figure 4.3, dollar welfare impacts peak in quarter six, fall steadily through the end of follow-up, but may continue to accrue some savings beyond year five. In passing, it may be noted that the quarterly patterns of AFDC impacts for AFDC-Us and AFDC-FGs overlap and track each other closely.

AFDC payments per month received (Table 4.4, italics) show a pattern for AFDC-Us that is different from the pattern for AFDC-FGs. When the monthly mean for controls is subtracted from the monthly mean for experimentals, the difference over the full follow-up period is $36 per month received, or 4.9 percent of the average monthly grant for controls on aid. This lesser monthly grant amount for experimentals remaining on AFDC accounts for half the total AFDC savings for AFDC-Us in the full follow-up.[10] The lower monthly grant amounts are observed consistently in

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[10] The number of months on AFDC for all experimentals, 27.79, times $36 per month equals $1,000, about half the total AFDC impact per AFDC-U enrollee. This calculation does not support a rigorous inference (continued...)

TABLE 4.5

SWIM

ALL AFDC-U: IMPACTS ON THE DISTRIBUTION OF EARNINGS DURING THE SECOND AND FIFTH YEAR OF FOLLOW-UP

	Quarters 6 - 9			Quarters 18 - 21		
Outcome	Experimentals	Controls	Difference	Experimentals	Controls	Difference
Average annual earnings (%)						
None	42.2	48.4	-6.2 **	51.2	55.0	-3.9
$1 - $1,999	17.6	14.1	3.4 *	11.1	7.0	4.1 ***
$2,000 - $4,999	12.3	10.1	2.2	7.5	7.3	0.2
$5,000 - $9,999	10.7	12.9	-2.2	8.5	8.3	0.2
$10,000 or more	17.3	14.5	2.8	21.7	22.3	-0.6
Total	100.0	100.0	0.0	100.0	100.0	-0.0
Average annual earnings, if employed (%) (b)						
$1 - $1,999	*30.4*	*27.4*	*3.0* (a)	*22.7*	*15.6*	*7.1* (a)
$2,000 - $4,999	*21.2*	*19.5*	*1.7* (a)	*15.3*	*16.2*	*-0.8* (a)
$5,000 - $9,999	*18.5*	*25.0*	*-6.5* (a)	*17.4*	*18.6*	*-1.1* (a)
$10,000 or more	*29.8*	*28.0*	*1.8* (a)	*44.5*	*49.7*	*-5.2* (a)
Total	*100.0*	*100.0*	*0.0*	*100.0*	*100.0*	*0.0*
Sample size (total = 1340)	686	654		686	654	

SOURCES AND NOTES: See Table 4.1.

(a) Not an experimental comparison; statistical tests not performed.

(b) Estimates in this panel are based only on persons with earnings. Statistical tests were not applied to the differences.

each of the five follow-up years. The range of dollar differences across years, excluding year one, is small, as is the range of percent differences. Given the tighter eligibility regulations for AFDC-Us -- especially the 100-hour and sanctioning rules -- this pattern is somewhat surprising. There is no clear explanation for it.

Table 4.6 shows little evidence of a deterrence effect for AFDC-Us. Over the full follow-up, the percentage of quarters not employed and not on AFDC was almost the same for experimentals and controls. The largest difference occurred in year three and was only 1.3 percentage points, half the peak-year effect for AFDC-FGs and not statistically significant. The table also shows an increase in the amount of time spent in the status "employed and received AFDC" (slightly larger than for AFDC-FGs), and this may be part of the explanation for the lesser monthly grant amounts for AFDC-U experimentals.

Conclusions regarding SWIM's impact on long-term AFDC-U stayers are the same as those discussed above for AFDC-FGs. As an aside, it may be noted that the AFDC-U sample was more likely to remain on AFDC a long time in the absence of SWIM. In particular, 40 percent of AFDC-U controls were still on welfare at the end of the follow-up period (quarter 22) compared to only 33.5 percent for AFDC-FG controls. Monthly grant amounts for AFDC-U controls still on aid in year five were $777, which exceeds the $604 monthly amount for AFDC-FG controls. The potential for AFDC savings and reductions in long-term AFDC receipt in the AFDC-U sample was therefore significant.

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[10] (...continued)
of causality, since, as noted above, the difference between conditional amounts is not a pure experimental difference.

TABLE 4.6

SWIM

ALL AFDC-U: IMPACTS ON COMBINED EMPLOYMENT AND AFDC RECEIPT STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference
Not employed, received AFDC (%)			
Average quarterly rate			
Quarters 2-21	33.4	37.5	-4.1 **
Quarters 2-5	47.6	53.8	-6.2 ***
Quarters 6-9	34.0	39.0	-5.0 **
Quarters 10-13	30.1	35.2	-5.1 **
Quarters 14-17	27.7	30.7	-3.0
Quarters 18-21	27.5	28.6	-1.1
Employed, received AFDC (%)			
Average quarterly rate			
Quarters 2-21	16.0	13.9	2.1 *
Quarters 2-5	21.5	17.6	3.8 **
Quarters 6-9	17.9	16.4	1.5
Quarters 10-13	15.1	12.7	2.4
Quarters 14-17	13.4	11.9	1.5
Quarters 18-21	11.9	10.8	1.1
Employed, did not receive AFDC (%)			
Average quarterly rate			
Quarters 2-21	23.4	21.8	1.6
Quarters 2-5	16.1	14.3	1.7
Quarters 6-9	23.9	20.5	3.3 *
Quarters 10-13	25.0	23.6	1.3
Quarters 14-17	26.8	25.6	1.1
Quarters 18-21	25.4	24.8	0.6
Not employed, did not receive AFDC (%)			
Average quarterly rate			
Quarters 2-21	27.2	26.9	0.4
Quarters 2-5	14.8	14.2	0.6
Quarters 6-9	24.2	24.0	0.2
Quarters 10-13	29.9	28.5	1.3
Quarters 14-17	32.1	31.7	0.4
Quarters 18-21	35.1	35.8	-0.7
Sample size (total = 1340)	686	654	

SOURCES AND NOTES: See Table 4.1.

CHAPTER 5

FIVE-YEAR BENEFIT-COST RESULTS

Benefit-cost analysis measures the overall gains and losses to SWIM eligibles, government budgets, taxpayers, and society as a whole. Gains and losses, like the impact estimates, represent *net* effects, with average outcomes for controls subtracted from average outcomes for experimentals. Benefit-cost analysis goes beyond the basic impact measures to include effects on fringe benefits, tax payments, Unemployment Insurance, Medi-Cal (California's Medicaid program), Food Stamps, the administrative costs associated with these transfer programs, the costs associated with operating SWIM and providing its services, and the value of output produced by SWIM participants in work experience assignments. With the exception of Unemployment Insurance benefits, these effects are imputed from observed impacts on employment, earnings, and AFDC receipt, in combination with published information about the eligibility rules and operating costs of the transfer programs in question and with data on the costs of operating SWIM and providing services.

The four groups for which gains and losses are calculated are termed the four benefit-cost "perspectives." These perspectives include those of AFDC applicants and recipients, government budgets, taxpayers, and society. The perspective of AFDC applicants and recipients identifies benefits and costs for members of the experimental group, indicating how they fared as a result of the program. The taxpayer perspective identifies benefits and costs from the standpoint of everyone in society other than individuals in the AFDC sample. The taxpayer and AFDC applicant-recipient perspectives together constitute the social perspective. The government budget perspective falls within the taxpayer perspective and measures the overall net effect of the program on federal, state, and local budgets.

Elements of benefits and costs do not all affect all groups, nor do they affect different groups in the same way. Effects that accrue as gains to one group may appear as losses to another. In addition, some limits on the comprehensiveness of the benefit-cost analysis should be recognized. In particular, the estimates below do not take into account possible long-term displacement of other workers by any increased employment of experimentals or the intangible benefits associated with society's preference for work over welfare. Finally, the benefit-cost results may be sensitive to the assumptions invoked in the analysis. A complete discussion of these assumptions and the benefit-cost methodology may be found in Hamilton and Friedlander (1989).

All benefit and cost formulas and parameters utilized in this report, as well as the underlying cost data, remain unchanged from the earlier report.[1] Any substantive differences between the estimates presented below and those of the earlier report therefore derive almost exclusively from the availability of three additional years of follow-up data on earnings and AFDC payments.[2] Whereas the earlier benefit-cost calculations were based on projections of short-term SWIM program impacts into years three, four, and five, the current calculations are based on actual data for those years. As a consequence, gains and losses directly related to impacts on earnings and AFDC payments may well differ in the two studies. Other gains and losses, such as tax payments, may change because the earnings and AFDC estimates on which they are based have been updated. Some costs, such as administrative expenses of transfer programs, are different in this report because the estimated use of those transfers has changed with the newly available five-year follow-up data. Estimates of the direct operating costs of SWIM, support services and allowances, and the use of community education and training programs are, however, largely unchanged.[3]

A. Results for the Single-Parent (AFDC-FG) Assistance Category

Table 5.1 presents the benefit-cost results for the AFDC-FG sample in SWIM. The results indicate that substituting actual five-year follow-up earnings and AFDC data for the projected

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[1] In the earlier report, benefit imputation algorithms were based on rates and rules in effect through 1988 for state and federal income taxes, the Earned Income Tax Credit, Food Stamps, and Medi-Cal. Some of these rates and rules have changed since 1988. However, the basic imputation formulas were not revised to take these recent changes into account, since these changes would result in estimates that would differ by only a small amount. Most problematic were cost increases in Medi-Cal, for which the figures in this report represent conservative estimates. More liberal allowance for Medi-Cal cost increases may have yielded estimates of budgetary savings of an additional $40 to $50 per experimental sample member. In addition, cost estimates in the earlier report were based on SWIM sample members' use of GAIN services according to early GAIN evaluation data available at the time of the 1989 report. As noted in Chapter 3, reviews of sample members' GAIN casefiles were conducted for this follow-up study in order to more precisely estimate use of these services. The cost estimates in this report were not revised to take into account these new GAIN data, since these changes would result in quite similar estimates. For example, the AFDC-FG net present value from the government perspective would increase by $46, and the AFDC-U net present value from the government perspective would increase by $118.

[2] The minor changes in sample definition and updating of data for years one and two, noted at the outset of the report, account for the remaining, very small differences.

[3] In the earlier report, some 17 AFDC-FGs and 47 AFDC-Us not in the present impact sample were included in the calculation of costs. These were dropped for this report in order to make impact and cost samples identical, thereby facilitating subgroup analysis. There result some small discrepancies between net cost estimates in this report and the earlier one.

TABLE 5.1

SWIM

ALL AFDC-FG: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS, BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	1800	0	-1800	0
Fringe benefits	216	0	-216	0
Output produced by participants				
EWEP	0	0	181	181
Employment	0	0	2016	2016
Tax payments				
Payroll taxes	-129	(b) 283	129	0
Income and sales taxes	34	-34	-34	0
Transfer programs				
AFDC payments	-1664	1664	1664	0
Payments from other programs	-202	202	202	0
Transfer administrative costs	0	37	37	37
SWIM operating costs	0	(c) -572	-572	-572
Support service and allowances	72	-72	-72	0
Use of community education and training programs	0	-249	-249	-249
Estimated GAIN costs	0	-27	-27	-27
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	126	1234	1260	1386

(continued)

Table 5.1 (continued).

SOURCE: MDRC calculations from the State of California Unemployment Insurance earnings and benefits records; AFDC payments records; the County of San Diego Department of Social Services SWIM Automated Tracking System and EWEP attendance logs; the San Diego Community College District Student Information System; the San Diego County JTPA Management Information System; MDRC time study of Department of Social Services and Employment Development Department staff; Employment Development Department participant cost records; county expenditure records; EWEP supervisor interviews; published data on transfer program administrative costs, tax rates, employee fringe benefits; GAIN casefile records; information gathered in interviews.

NOTES: Results are expressed in 1986 dollars. Differences are regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members. The AFDC-FG sample includes 1604 experimentals and 1606 controls, and the AFDC-U sample includes 686 experimentals and 654 controls. Because of rounding, details may not sum to totals.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling $2,153, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $920, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $2.34.

amounts of the previous SWIM report (Hamilton and Friedlander, 1989) did not result in large changes in the overall benefit-cost findings. The additional follow-up data suggest that, taking various financial effects into account, AFDC-FG sample members approximately broke even; AFDC-U sample members showed a modest net loss; and savings to government budgets greatly exceeded net program costs.[4]

Table 5.1 shows four columns, corresponding to the four benefit-cost perspectives. Within each perspective, the rows of the table give the estimates of the gains or losses associated with each of several program effects. Effects can appear with different signs in different columns. Zeroes indicate that a particular effect is neither a gain nor a loss from a given perspective. Some intangible effects, which could not be measured, are shown with just a plus or minus sign to indicate how they would modify the total for a perspective.

The dollar entries of the table are stated as *net present values per experimental sample member*. This means, first, that the amounts represent program effects on average for an individual SWIM enrollee and do not represent the total effect for the whole program. Second, "net" means that the amounts represent differences between experimentals and controls, just as impacts do. In fact, "net earnings" is only another way of saying "earnings impact." Third, "present value" is an accounting method for estimating the worth today of dollar effects that occur in the future. A present value is a single dollar amount which, if allowed to accrue interest, would exactly substitute for a stream of future dollar amounts. The present value computation therefore permits direct comparison of amounts accruing near the time of enrollment, such as program operating expenses, and amounts accruing later, such as earnings impacts and welfare impacts. Present values over a five-year period are generally less than the simple sums of the future dollar impacts. Thus, the present value of earnings impacts in Table 5.1 is less than the impact on "average total earnings" over the full follow-up period, shown in Table 4.1, even though the two amounts summarize exactly the same quarter-by-quarter impacts on earnings. Finally, all benefits and costs are expressed in 1986 dollars.

The first entry in the table is the present value of earnings impacts. For AFDC-FGs, this amounted to $1,800, which, with the addition of $216 imputed for fringe benefits, brings a benefit of

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[4] As noted in Chapter 3, some of SWIM's benefits may continue beyond the fifth year of follow-up. This implies that a benefit-cost time horizon longer than five years may be appropriate, using five years of actual data and projections for later years. Resources available for this study did not permit such a calculation. However, the five-year impact estimates suggest that a benefit-cost analysis using a longer time horizon would yield results similar to those obtained with the five-year data.

$2,016 per experimental sample member. Against this are weighed small increases or decreases in tax payments. Also subtracted is the amount of $1,664, the present value of reductions in AFDC income, and $202 for other transfers. A small amount for support services and allowances from SWIM is then added in to give the total net present value of $126 for the perspective of the welfare sample.[5] (Note that, owing to rounding, details will not always sum exactly to totals.)

This bottom-line amount is similar to the range of net present value projections made in the earlier, two-year follow-up report. The component gains and losses are somewhat different, however. Because the actual amounts of impacts on earnings and AFDC during follow-up years three, four, and five are less than the earlier projections, the present values of gains in earnings and losses in transfers are both lower by $200 to $500, and these simultaneous changes offset each other.

The second column in Table 5.1 shows the government budget perspective for all levels of government. Enrollee earnings are not counted, but tax payments do accrue here, including the employer share of social security taxes, which was not counted in the welfare sample perspective. The main gain for government budgets is the savings in AFDC and other transfers. The main losses are SWIM operating costs, community education and training program costs, and other program costs. These amounts are the same as those of the previous report and together sum to $920, which may be considered the net cost of running SWIM.[6] Subtracting these costs from the net gains ($2,153) gives the total net present value of $1,234 for the government budget perspective. This amount is $300 to $600 less than the range projected in the earlier report, owing to the lower amount of welfare impacts in years three through five. The total is still the largest "profit" estimated for government budgets for a broad-coverage, experimentally evaluated welfare-to-work program and represents a return of $2.34 for every dollar invested.

The taxpayer perspective, shown in column three of the table, is similar to the government budget perspective. The chief difference is the addition of an imputed amount for the value of SWIM participants' work in their unpaid (EWEP) work assignments in public or nonprofit institutions. Also, the employer share of social security taxes is excluded. The estimated total net present value to taxpayers is slightly larger than the total for government budgets.

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[5] Note that in Table 5.1, the application of federal income tax regulations resulted in AFDC-FG sample members paying *lower* taxes than their control counterparts. This is due to eligibility rules for the federal Earned Income Tax Credit (EITC). Families with annual earnings lower than a fixed amount can receive the EITC; those with no earnings or with earnings above the limit are not eligible.

[6] Among experimentally evaluated, JOBS-predecessor programs, only one – the Baltimore Options Program – has equalled these levels of resources. Net costs for Options were estimated as $953 (Gueron and Pauly, 1991, p. 171). This program had slots for only 1,000 participants at the time it was evaluated, making it a smaller-scale program than SWIM. Cost data are not yet available from the evaluation of California's GAIN program.

The social perspective, the last column in the table, is defined as the sum of the welfare sample and taxpayer perspectives. This perspective counts only the value of real increases in goods and services against real resources expended. Taxes and transfers are not counted, since they do not represent resources used up or services created.[7] This perspective assumes that a dollar gained or lost by AFDC applicants and recipients has the same social value as a dollar gained or lost by taxpayers, and that there is no distributional preference from society's perspective. The total social net present value of SWIM is estimated at $1,386 per experimental sample member.

B. Results for the Two-Parent (AFDC-U) Assistance Category

Table 5.2 presents the benefit-cost results for AFDC-Us. The methodology and organization of the table are the same as for AFDC-FGs, with the exception that most child care costs in SWIM were allocated to AFDC-FGs.

The AFDC-U welfare sample suffered a net loss of $593 over the five-year follow-up period. Reductions in AFDC payments exceeded the gains in earnings plus fringe benefits. In the previous report, a two-year follow-up projected a smaller total loss, less than $100. But the narrowing of the experimental-control differential in earnings after follow-up year two was considerably faster than originally projected. Thus, the five-year present value of earnings impacts estimated with extended follow-up data is only about half the original five-year projection. The present value of AFDC reductions was also smaller than earlier projected, but the difference was much less than for earnings.

From the government budget perspective, total gains were estimated as $1,188 per AFDC-U experimental, almost the same as for AFDC-FGs. As indicated for AFDC-FGs, this is a relatively large return ($2.41 for every dollar invested), even though the amount is $500 to $800 less than projected with the two-year follow-up data.

The taxpayer net present value is somewhat greater than the government budget net present value, which was the case for AFDC-FGs. From the social perspective, the net present value is positive, but the $772 amount is only about half the amount for AFDC-FGs. The reason for this difference is the more rapid narrowing of the experimental-control difference in earnings for AFDC-Us during years three, four, and five.

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[7] Earnings and fringe benefits from employment of the research sample is taken to be the value of goods and services created by them in their jobs.

TABLE 5.2

SWIM

ALL AFDC-U: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS, BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	1125	0	-1125	0
Fringe benefits	135	0	-135	0
Output produced by participants				
EWEP	0	0	274	274
Employment	0	0	1260	1260
Tax payments				
Payroll taxes	-81	178	81	0
Income and sales taxes	-70	70	70	0
Transfer programs				
AFDC payments	-1700	1700	1700	0
Payments from other programs	-50	50	50	0
Transfer administrative costs	0	29	29	29
SWIM operating costs	0	-569	-569	-569
Support service and allowances	49	-49	-49	0
Use of community education and training programs	0	-202	-202	-202
Estimated GAIN costs	0	-20	-20	-20
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	-593	1188	1365	772

(continued)

Table 5.2 (continued).

SOURCES AND NOTES: See Table 5.1.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling $2,028, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $840, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $2.41.

CHAPTER 6

SUBGROUP ESTIMATES

In this chapter, differences between experimentals and controls are investigated within a number of subgroups of the AFDC-FG and AFDC-U samples. One purpose is to provide guidance for targeting policy. In this connection, comparisons can be made with an earlier study by Friedlander (1988), which examined subgroup impacts for five low- to moderate-cost, broad-coverage welfare-to-work programs of the 1980s. To summarize the present findings, no single subgroup emerged as clearly the best performer, a candidate for exclusive attention by future programs. Instead, there is evidence of earnings gains and AFDC reductions for a variety of subgroups in SWIM, and also differences between the SWIM subgroup impacts and those of the earlier subgroup study. Such variation in subgroup impacts is consistent with a broad, inclusive targeting approach rather than a narrow one – a conclusion of both the SWIM study and the earlier subgroup study. Particularly where AFDC reductions are an important policy goal, the evidence favors SWIM's inclusionary approach, working with the full spectrum of the eligible population, from the most "job ready" to the most disadvantaged, without subjective program entry criteria. Even the relatively disadvantaged portions of the samples, for which the highest net costs were incurred, reached at least the break-even point for government budgets.

Statistically significant earnings and AFDC impacts were found for several subgroups in the AFDC-FG sample. There is some indication that earnings impacts were generally greater for AFDC-FG subgroups facing fewer barriers to employment or with shorter AFDC histories, but this pattern was not uniform and the *difference* in impacts across subgroups was usually not statistically significant. For example, earnings impacts were larger for individuals who enrolled in SWIM already possessing a high school diploma or GED than for those without these credentials. However, although first-time AFDC applicants had relatively large earnings impacts, less disadvantaged AFDC recipients had higher earnings impacts than did applicants returning to the AFDC rolls. The pattern of AFDC-FG subgroup impacts on AFDC payments was different. Statistically significant AFDC impacts were observed for most subgroups. AFDC impacts were distributed fairly evenly across some subgroup dimensions, although prior earnings and the length of time the individual had her or his own AFDC case showed AFDC impacts increasing from the less to the more disadvantaged.

The patterns of impacts on earnings and AFDC also differed among AFDC-U subgroups. While many AFDC-U subgroups experienced earnings impacts, these impacts tended to be larger for subgroups facing fewer employment barriers, although few of the AFDC-U subgroup impacts on earnings were statistically significant and none of the *differences* in earnings impacts across subgroups were statistically significant. For example, earnings impacts were greater for individuals with higher, as compared to lower, earnings in the year prior to SWIM enrollment. AFDC-U subgroup impacts on AFDC payments were more likely to be statistically significant and exhibited a clearer pattern. For this outcome measure, impacts were found only among the more disadvantaged subgroups, e.g., individuals with small to no prior year earnings, those lacking a high school diploma or GED, enrollees with at least some previous experience receiving AFDC, and AFDC recipients considered to be more disadvantaged. It is notable that several of the differences across AFDC-U subgroups in impacts on AFDC payments were statistically significant.

At the same time, however, the ratio of earnings gains to AFDC reductions for a number of the relatively disadvantaged subgroups was low, leaving those subgroups worse off financially, at least as far as their own Unemployment Insurance-reported earnings and AFDC income. In every case, both for AFDC-FGs and AFDC-Us, earnings gains during the five-year follow-up period were less than the corresponding AFDC payment reductions for subgroups without recent earnings, without a high school diploma, with more than two children, with more than two years on their own AFDC case, and for a "most disadvantaged" subgroup defined as recipients having no recent earnings, more than two years on AFDC, and no diploma.

Relationships among subgroup estimates give us some evidence about the underlying mechanisms of program effect, although causality cannot be established rigorously. With regard to earnings impacts, it was observed that the SWIM subgroup impacts do not show a strong correlation between incremental participation and earnings gain. Subgroups with high (low) incremental in-program activity were often not those with a large (small) earnings impact. Incremental activity appeared higher for subgroups with higher labor market barriers and with longer AFDC histories, but these subgroups did not consistently show above-average earnings gains. This may mean that mechanisms other than participation are also important in producing impacts. Alternatively, it may mean that greater effort (i.e., a higher level of participation) is required to produce a given earnings gain among the "less job ready" subgroups.[1]

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[1] It could also be that impacts on nonparticipants differ across subgroups.

With regard to AFDC impacts, some evidence was found that sanctioning did not directly produce large AFDC reductions. In particular, there were no large AFDC reductions accruing to subgroups with the highest sanctioning rates. But there was also not a strong correlation across subgroups between earnings gains and AFDC reductions. That result is consistent with the relative cost hypothesis, i.e., that their SWIM experiences may have lead some experimentals to see less value in remaining on (or returning to) AFDC.

Subgroups based on ethnicity were also examined. There is evidence that SWIM achieved some success in working with Hispanic experimentals. Both AFDC-FG and AFDC-U Hispanic experimental sample members had incremental participation rates that were as large as or larger than those of other ethnic groups, and they experienced increases in education and training (relative to the control group) that were substantially greater than those for non-Hispanic whites or blacks. Impact estimates for Hispanics showed above-average earnings gains, although the impact differences across ethnic subgroups were not statistically significant. Hispanics also obtained AFDC reductions: The amounts were not large enough to offset their observed earnings gains, but were significant from the government budget point of view. In looking at the SWIM experiences of other ethnic groups, sanctioning rates were found to be higher for non-Hispanic blacks, but this group did not have particularly large impacts on AFDC income as a consequence.

Finally, there was an examination of four subgroups defined on the basis of whether they were currently or previously active in employment-directed activities or employed at the time of random assignment. Two of these subgroups, encompassing those active in education or training as of random assignment, are often referred to as "self-initiated participants," i.e., individuals who have already sought out education or training programs, on their own initiative, prior to enrolling in welfare-to-work programs. Programs differ in their treatment of these individuals. Some programs, such as JOBS, specify that these persons, if they are within the JOBS target groups, should be given priority over other enrollees in receiving child care services and case management. Other programs, such as SWIM, have not given these individuals specific priority for such services (although in SWIM, no priorities were necessary because resources were available to serve all enrollees equally), and have required these individuals to participate in the program's regular array of components if they complete or drop out of their self-initiated activity. The results indicate that SWIM impacts for these subgroups were generally average or above average, even though incremental participation estimates for these subgroups were often relatively low. This suggests that other elements of SWIM – the program's message emphasizing work over welfare, SWIM's case management, the close monitoring of experimentals' activities, or the requirement that individuals participate in job search and unpaid work experience if they dropped out of their self-initiated programs – possibly played important roles in producing impacts for these groups.

A. Rationale and Analysis Issues

Subgroup analysis applies the fundamental experimental-control comparison to sample members who, just prior to random assignment, have a single characteristic or set of characteristics in common. For example, increases in education and training activity for the subgroup of long-term AFDC recipients can be estimated by comparing the average duration of those activities for only experimentals and controls who, at the time of random assignment, reported having received AFDC on their own case for more than two years. Thus, the strength of the experimental design, which stems from comparisons of individuals with similar characteristics, is preserved in the subgroup analysis.

Subgroup analyses for large-scale welfare-to-work program evaluations typically focus on subgroup differences in impacts. Of particular interest are subgroups having suspected barriers to employment or subgroups with a long history of welfare receipt. These subgroups are traditionally thought of as the most challenging and expensive to work with, but they are also the subgroups with the greatest room for change and, hence, the greatest potential for long-term impact. Large impacts with these subgroups increase the value of a particular program. Small impacts for these subgroups limit the total program effect. Some of these groups were specifically targeted for services in the JOBS legislation.

In SWIM, subgroup analysis can address additional questions because participation data are available for controls as well as for experimentals. Using these data, it can be determined whether subgroups with large (or small) impacts were also subgroups for which the program most (or least) increased participation in job search, unpaid work assignments, or education and training. Conversely, it can also be determined whether high (or low) impact subgroups were those for which the program obtained the highest (or lowest) rates of sanctioning and coverage. Estimates of differences in participation, sanctioning, and coverage across subgroups may identify which of these elements of SWIM might be most or least effective, may help explain the distribution of impacts over the sample, and may suggest possible future changes in program design and targeting strategies. For example, if subgroups with the highest rates of sanctioning did not have particularly large AFDC reductions, then one might question the efficacy of sanctions in producing welfare savings directly. One would not know, however, whether sanctions or the threat of sanctions was important in securing compliance with the participation requirement.

The association of measures across subgroups, or the absence of an expected association, is often difficult to interpret, however. Subgroup differences in treatment – e.g., the hypothetical difference in sanctioning – are not the result of planned manipulation by the experiment, and so causality cannot be established rigorously. Two or more competing explanations may fit the same observed pattern. Or a particular mechanism may actually be effective without producing the expected pattern. Caution must be maintained in drawing conclusions. As an illustration, consider the hypothesis that program services should be effective in increasing earnings. Under this hypothesis, one should expect a positive correlation across subgroups between incremental participation and impact. Such a positive correlation might not be observed under two circumstances: (1) if elements of SWIM other than participation contributed strongly to impacts or (2) if incremental participation was greatest among subgroups with which the program had the most difficulty achieving impacts. In the event a weak correlation between incremental participation and impact is found, it may be difficult to distinguish which of these two possible explanations is correct.

A number of statistical issues must be dealt with in interpreting subgroup estimates. Subgroup estimates are, obviously, based on smaller samples than are the corresponding full-sample estimates. Thus, individual subgroup estimates are less precise and more susceptible to spurious variation or "noise." Experimental-control differences of a given magnitude that were statistically significant for the full sample will often not be statistically significant for subgroups. This is particularly true for estimates of impacts on earnings.

Subgroup analysis also makes use of additional statistical test procedures. In particular, one is interested not only in testing the basic experimental-control differences, but also in testing the difference between these differences across subgroups. For example, if one subgroup has a larger numerical impact estimate than another, it would be important to rule out chance as a factor in producing that larger impact. More generally, if several subgroups have different numerical impact estimates, one would want to rule out chance as a factor in producing that entire set of differences. For this purpose, the results of F-tests for differences in subgroup differences will be reported here. Each F-test result is shown in the tables above the set of subgroup estimates to which it pertains.

Statistically significant results mean that the differences among a group of subgroup experimental-control differences probably did not stem from chance.

There is also the complex problem of correctly interpreting subgroup estimates when there are several subgroups. The large number of estimates produced by such an analysis increases the odds that some of the estimates may be large or small by chance alone rather than by real factors related to the program. To overcome this "multiple comparisons" problem, one usually looks for consistent patterns of results across several subgroups and will not generally emphasize individual estimates that are not backed up by others supporting similar conclusions. It is necessary to be quite cautious about concluding that one or another specific subgroup necessarily will have larger or smaller program impacts in similar kinds of programs just because the impact estimates for that subgroup in SWIM were above or below average.

B. Subgroup Findings

Tables 6.1 through 6.4 present subgroup estimates for AFDC-FGs and AFDC-Us. These estimates are "unconditional," i.e., they show differences for each subgroup without taking into account other characteristics that may be correlated with membership in that subgroup. For example, impact estimates for sample members without a high school diploma do not take into account the weaker recent employment of that subgroup.

Participation estimates are shown first, followed by impact estimates. This analysis organizes subgroups for SWIM to investigate several broad areas: possible program implementation differences across time and space; "barriers to employment"; welfare history; ethnicity; and whether sample members had current or prior experiences in program activities at the time of random assignment. These areas are arrayed in the same order from the top to the bottom of each table. For each subgroup within these broad areas, the percentage of the full sample in the subgroup is shown, as are the experimental and control group means, the experimental-control difference, and the statistical significance of that difference. Above each set of subgroup estimates, the statistical significance of the differences among subgroups in that set is shown. Where applicable, the subgroups are ordered within each set with those for which controls have the highest rates of future employment and welfare exit at the top and those with the lowest rates at the bottom.

Table 6.1 presents subgroup participation estimates for AFDC-FGs; Table 6.2 presents estimates for AFDC-Us. The first variable analyzed is the overall rate of participation in any pre-GAIN activity, including SWIM job search assistance and unpaid work experience, and education

TABLE 6.1

SWIM

AFDC-FG SUBGROUPS: ACTIVITY ESTIMATES

Subgroup	Percent of Sample	Ever Participated in Any Activity (%) Experi-mentals	Controls	Difference	(b)	Days Enrolled in Education or Training Experi-mentals	Controls	Difference	(b)	Percent of Experimentals (c) Sanc-tioned	Not Covered
FULL SAMPLE	100.0	69.4	30.0	39.4	***	82.2	58.9	23.3	***	11.4	5.8
IMPLEMENTATION											
SWIM office				xxx							x
Service Center	49.9	71.2	26.4	44.8	***	77.7	51.9	25.9	***	11.8	6.8
San Diego West	50.1	67.5	33.5	34.0	***	86.7	65.9	20.8	***	10.9	4.8
Random assignment cohort											xx
July-September 1985	29.6	72.8	32.8	40.0	***	93.1	65.0	28.1	***	9.7	4.5
October-December 1985	25.2	70.1	28.6	41.5	***	86.1	60.7	25.3	**	12.4	3.8
January-March 1986	24.3	69.3	30.9	38.4	***	76.5	62.2	14.3		13.7	7.1
April-June 1986	21.0	64.0	26.6	37.3	***	69.5	43.8	25.7	**	9.7	8.6
BARRIERS TO EMPLOYMENT											
Prior year earnings				xxx							xxx
$3,000 or more	17.7	67.2	35.5	31.7	***	89.6	75.0	14.6		9.9	1.7
$1-$2,999	21.6	68.8	35.4	33.5	***	78.0	60.7	17.3		11.6	3.4
None	60.7	70.2	26.4	43.8	***	81.7	53.3	28.4	***	11.7	8.0
High school diploma				xx				x			xx
Yes	56.1	70.1	34.1	36.0	***	82.7	67.8	14.9	**	12.4	4.7
No	43.9	68.5	24.8	43.7	***	81.7	47.6	34.1	***	10.1	7.3
Number of own children (a)				xxx				x			
One	45.7	67.9	30.7	37.2	***	70.7	58.1	12.6	*	11.0	6.1
Two	29.7	73.4	25.6	47.8	***	93.9	52.7	41.2	***	12.1	4.6
More than two	20.2	73.1	33.5	39.5	***	90.5	66.3	24.2	**	12.3	7.1

(continued)

Table 6.1 (continued).

Subgroup	Percent of Sample	Ever Participated in Any Activity (%)				Days Enrolled in Education or Training				Percent of Experimentals (c)	
		Experimentals	Controls	Difference	(b)	Experimentals	Controls	Difference	(b)	Sanctioned	Not Covered
AFDC HISTORY AND STATUS											
Had own AFDC case				xxx						xx	xxx
Never	11.2	65.6	34.9	30.7	***	72.6	58.6	14.0		5.0	1.6
Two years or less	20.2	63.0	31.0	32.0	***	73.3	66.1	7.2		13.8	3.4
More than two years	68.6	71.9	29.0	42.9	***	86.5	56.9	29.5	***	11.7	7.2
Welfare status				x				xx		xx	xxx
Applicant	39.2	64.4	28.8	35.5	***	66.1	56.4	9.7		13.6	3.3
Recipient	60.8	72.7	30.8	41.9	***	92.9	60.8	32.1	***	9.9	7.4
Level of disadvantage				xxx						xxx	xxx
First-time applicants	8.6	70.0	31.0	38.9	***	62.8	53.4	9.5		5.4	1.0
Returning applicants	30.6	62.8	28.2	34.5	***	67.0	57.2	9.8		15.9	4.0
Less disadvantaged recipients	42.4	72.0	34.7	37.3	***	94.2	65.8	28.4	***	10.5	6.2
More disadvantaged recipients	18.4	74.6	22.2	52.4	***	90.4	49.3	41.1	***	8.5	9.7
ETHNICITY				xxx				xxx		xxx	
White, non-Hispanic	27.3	59.0	28.7	30.4	***	61.3	53.9	7.4		9.0	5.7
Black, non-Hispanic	42.4	74.5	33.3	41.2	***	78.7	62.2	16.6	**	16.4	5.5
Hispanic	25.4	71.7	24.8	46.9	***	99.4	53.4	46.0	***	7.0	6.1
Asian and other	4.9	74.1	34.2	39.8	***	157.1	80.9	76.2	***	2.9	8.2

(continued)

Table 6.1 (continued).

Subgroup	Percent of Sample	Ever Participated in Any Activity (%)				Days Enrolled in Education or Training				Percent of Experimentals (c)	
		Experi-mentals	Controls	Difference	(b)	Experi-mentals	Controls	Difference	(b)	Sanc-tioned	Not Covered
SPECIAL SUBGROUPS											
Hispanic: English speaking				(e)				(e)		(e)	(e)
Yes	17.4	70.3	24.2	46.1	***	97.8	44.8	53.0	***	8.9	5.2
No	8.0	74.5	26.1	48.5	***	103.2	72.1	31.1	*	3.0	8.2
Activity status at random assignment (d)				(e)				(e)		(e)	(e)
In school	14.9	74.9	61.7	13.2	***	194.9	178.5	16.4		3.6	5.4
In any activity	15.5	75.7	60.4	15.3	***	183.6	166.1	17.5		6.0	4.5
Previously in program activity	37.1	75.5	38.8	36.7	***	117.0	81.3	35.7	***	9.9	5.2
Employed more than 20 hours per week	5.4	52.0	34.8	17.2	**	82.4	59.9	22.5		4.6	6.0
Sample size (total = 3210)		1604	1606			1604	1606			1604	1604

SOURCES AND NOTES: See Table3.1.

(a) Results for a fraction of the sample reporting zero "own children" are excluded from the table.

(b) An F-test was applied to subgroup differences in "difference" estimates. Statistical significance levels are indicated as: x = 10 percent; xx = 5 percent; xxx = 1 percent.

(c) An F-test was applied to subgroup differences in "Percent of Experimentals Sanctioned" and "Percent of Experimentals Not Covered." Statistical significance levels are indicated as: x = 10 percent; xx = 5 percent; xxx = 1 percent.

(d) These categories are not mutually exclusive and do not include sample members with no activity status at random assignment.

(e) F-test not applied.

TABLE 6.2

SWIM

AFDC-U SUBGROUPS: ACTIVITY ESTIMATES

		Ever Participated in Any Activity (%)				Days Enrolled in Education or Training				Percent of Experimentals (c)	
Subgroup	Percent of Sample	Experi-mentals	Controls	Difference	(b)	Experi-mentals	Controls	Difference	(b)	Sanc-tioned	Not Covered
FULL SAMPLE	100.0	70.2	23.2	47.1	***	66.5	42.5	24.0	***	9.6	3.7
IMPLEMENTATION											
SWIM office				xx							
Service Center	50.7	72.6	19.5	53.2	***	53.7	27.1	26.7	***	9.8	4.5
San Diego West	49.3	67.7	27.0	40.7	***	79.7	58.5	21.2	**	9.3	2.9
Random assignment cohort											
July-September 1985	29.4	68.8	28.5	40.3	***	71.4	45.9	25.5	*	8.9	3.5
October-December 1985	24.6	70.4	21.0	49.4	***	66.8	41.2	25.6	*	8.8	4.1
January-March 1986	23.1	69.1	20.3	48.8	***	66.4	43.3	23.1		11.7	6.0
April-June 1986	22.9	73.0	21.6	51.5	***	59.7	39.1	20.6		9.0	1.2
BARRIERS TO EMPLOYMENT											
Prior year earnings											xxx
$3,000 or more	32.1	67.4	26.7	40.7	***	55.3	43.9	11.4		9.1	0.9
$1-$2,999	24.5	72.7	20.9	51.8	***	57.1	34.2	22.9		13.6	1.5
None	43.4	71.0	21.8	49.2	***	80.5	46.1	34.4	***	7.6	7.0
High school diploma				x							xxx
Yes	47.0	66.8	24.3	42.5	***	58.0	39.4	18.7	*	10.9	1.6
No	53.0	73.1	22.0	51.1	***	74.0	45.3	28.7	***	8.4	5.6
Number of own children (a)											
One	26.5	69.8	15.2	54.6	***	51.4	18.9	32.5	**	11.8	1.3
Two	31.3	69.9	23.0	46.9	***	67.2	27.0	40.3	***	11.7	4.2
More than two	39.9	71.1	29.2	41.9	***	79.7	73.1	6.5		6.8	4.7

(continued)

Table 6.2 (continued).

Subgroup	Percent of Sample	Ever Participated in Any Activity (%) Experi-mentals	Controls	Difference	(b)	Days Enrolled in Education or Training Experi-mentals	Controls	Difference	(b)	Percent of Experimentals (c) Sanc-tioned	Not Covered
AFDC HISTORY AND STATUS											
Had own AFDC case											
Never	33.7	65.6	24.1	41.5	***	54.8	35.6	19.2		11.0	1.9
Two years or less	34.6	70.7	22.9	47.8	***	56.3	33.7	22.5	*	8.9	4.0
More than two years	31.8	74.6	22.6	52.0	***	89.9	59.9	30.0	**	8.8	5.1
Welfare status											xxx
Applicant	59.5	68.8	21.4	47.4	***	48.0	29.1	18.8	**	10.3	1.5
Recipient	40.5	72.3	25.8	46.5	***	93.5	62.0	31.5	***	8.5	7.1
Level of disadvantage											xxx
First-time applicants	30.6	65.3	23.8	41.5	***	49.5	33.0	16.5		11.0	2.0
Returning applicants	28.9	72.7	19.4	53.3	***	45.9	25.8	20.1		9.5	0.8
Less disadvantaged recipients	29.3	73.1	26.6	46.4	***	90.6	50.5	40.1	***	9.6	6.7
More disadvantaged recipients	11.2	70.2	23.8	46.4	***	100.8	90.2	10.6		5.2	7.7
ETHNICITY				xxx						xxx	
White, non-Hispanic	25.6	61.2	20.5	40.6	***	29.8	23.8	6.0		9.1	3.1
Black, non-Hispanic	20.8	71.2	13.2	58.0	***	53.2	25.5	27.6	*	16.3	3.2
Hispanic	40.2	74.7	21.5	53.3	***	75.4	34.8	40.6	***	8.5	3.6
Asian and other	13.4	72.4	47.2	25.2	***	134.7	125.1	9.6		2.5	6.1

(continued)

Table 6.2 (continued).

Subgroup	Percent of Sample	Ever Participated in Any Activity (%) Experimentals	Controls	Difference	(b)	Days Enrolled in Education or Training Experimentals	Controls	Difference	(b)	Percent of Experimentals (c) Sanctioned	Not Covered
SPECIAL SUBGROUPS											
Hispanic: English speaking				(e)				(e)		(e)	(e)
Yes	26.9	75.6	24.5	51.2	***	62.7	43.9	18.9		10.6	1.4
No	13.3	72.4	16.4	56.0	***	103.7	20.3	83.4	***	3.6	8.2
Activity status at random assignment (d)				(e)				(e)		(e)	(e)
In school	8.9	73.5	54.0	19.5	**	177.1	159.2	17.9		0.0	3.8
In any activity	10.3	74.5	56.0	18.5	**	166.2	151.0	15.2		-0.2	5.2
Previously in program activity	29.6	76.8	33.8	43.0	***	92.2	73.2	19.0		8.7	3.5
Employed more than 20 hours per week	3.2	41.0	28.4	12.6		45.8	44.6	1.2		2.9	3.1
Sample size (total = 1340)		686	654			686	654			686	686

SOURCES AND NOTES: See Table 3.1.

(a) Results for a fraction of the sample reporting zero "own children" are excluded from the table.

(b) An F-test was applied to subgroup differences in "difference" estimates. Statistical significance levels are indicated as: x = 10 percent; xx = 5 percent; xxx = 1 percent.

(c) An F-test was applied to subgroup differences in "Percent of Experimentals Sanctioned" and "Percent of Experimentals Not Covered." Statistical significance levels are indicated as: x = 10 percent; xx = 5 percent; xxx = 1 percent.

(d) These categories are not mutually exclusive and do not include sample members with no activity status at random assignment.

(e) F-test not applied.

and training. The table gives the experimental and control group means as well as the experimental-control difference in this "ever participated" measure. This difference is the "incremental participation" highlighted in Chapter 3, and it tells what the program accomplished for each subgroup, above and beyond what the subgroup would have done on its own. It should be recalled that controls, as planned, received almost no formal job search assistance or unpaid work assignments, but many did seek out and participate in education and training activities in the community.

The next variable is the number of days enrolled in education and training, and this is also shown with research group means as well as in incremental form (i.e., as an experimental-control difference). Then sanctioning rates are presented. Although these are given as simple sanctioning rates for experimentals, the estimates are virtually the same as experimental-control differences because controls received very few sanctions. The last variable is the percentage of experimentals not covered, i.e., the percentage of a subgroup that remained on AFDC and jobless, did not participate in a formal activity, and was never sanctioned. Sample members who were not covered are those for whom the mandate to "participate, work, or leave welfare" was apparently not applied. Coverage is not shown as an experimental-control difference, but rather as a percentage of experimentals alone, since the main interest of the analysis is in learning whether there are any subgroups into which the SWIM treatment might have penetrated further.

Tables 6.3 and 6.4 present estimates of subgroup impacts on earnings and AFDC payments over the full follow-up period. Appendices B and C give estimates of subgroup impacts on earnings in year two and earnings in year five to show patterns over time. For the same purpose, estimates of subgroup impacts on AFDC payments in years two and five are also given in the Appendices.

1. Program Implementation Across Office and Time. In the subgroup tables, the first major analysis category is "program implementation" -- possible differences between the two SWIM offices (Service Center and San Diego West) or across cohorts of sample members that entered SWIM in each three-month interval of the demonstration period. The purpose is to find any differences in incremental activity that might then relate to differences in impact. A difference in incremental activity is found in the "ever participated" estimates for the two SWIM offices. The increment for Service Center was greater than for San Diego West, both for AFDC-FGs and AFDC-Us, and the differences were statistically significant. Duration of education and training was also somewhat greater for the Service Center for both assistance categories, but the differences were not as great and were not statistically significant. These cross-office differences were the principal observed differ-

TABLE 6.3

SWIM

AFDC-FG SUBGROUPS: IMPACTS ON EARNINGS AND AFDC PAYMENTS

		Average Total Earnings Quarters 2-21 ($)				Average Total AFDC Payments Quarters 2-21 ($)			
Subgroup	Percent of Sample	Experi-mentals	Controls	Difference	(b)	Experi-mentals	Controls	Difference	(b)
FULL SAMPLE	100.0	16109	14033	2076	**	15726	17642	-1916	***
IMPLEMENTATION									
SWIM office									
Service Center	49.9	14887	13792	1095		17500	19163	-1663	***
San Diego West	50.1	17336	14283	3053	**	13957	16124	-2167	***
Random assignment cohort									
July-September 1985	29.6	15603	13959	1645		15586	17573	-1988	**
October-December 1985	25.2	15131	14435	696		14613	16449	-1836	**
January-March 1986	24.3	16408	13812	2597		17270	18227	-957	
April-June 1986	21.0	17596	13917	3679	**	15463	18521	-3058	***
BARRIERS TO EMPLOYMENT									
Prior year earnings									
$3,000 or more	17.7	31079	27267	3812	*	11641	12440	-800	
$1-$2,999	21.6	16746	15858	887		14982	16614	-1632	*
None	60.7	11518	9537	1982	*	17198	19531	-2332	***
High school diploma									
Yes	56.1	20540	18135	2405	**	13904	15945	-2040	***
No	43.9	10437	8783	1654		18059	19815	-1756	***
Number of own children (a)									
One	45.7	17321	14471	2849	**	11765	14413	-2647	***
Two	29.7	16541	14814	1727		16681	17763	-1082	
More than two	20.2	13785	12034	1752		23446	25468	-2022	**

(continued)

Table 6.3 (continued).

Subgroup	Percent of Sample	Average Total Earnings Quarters 2-21 ($)				Average Total AFDC Payments Quarters 2-21 ($)			
		Experi-mentals	Controls	Difference	(b)	Experi-mentals	Controls	Difference	(b)
AFDC HISTORY AND STATUS									
Had own AFDC case									
Never	11.2	21030	15806	5225	**	11128	12497	-1369	
Two years or less	20.2	21659	19108	2551		10698	12429	-1731	*
More than two years	68.6	13651	12237	1414		17949	20020	-2072	***
Welfare status									
Applicant	39.2	17911	17030	881		11206	13046	-1841	***
Recipient	60.8	14968	12122	2845	***	18635	20599	-1964	***
Level of disadvantage				x					
First-time applicants	8.6	22483	16285	6198	**	9002	11215	-2213	
Returning applicants	30.6	16620	17242	-622		11811	13577	-1767	**
Less disadvantaged recipients	42.4	18247	14833	3415	***	17484	19499	-2015	***
More disadvantaged recipients	18.4	7332	5850	1483		21218	23201	-1983	**
ETHNICITY									
White, non-Hispanic	27.3	17963	16546	1417		11803	14134	-2331	***
Black, non-Hispanic	42.4	14846	13926	920		17250	18856	-1606	**
Hispanic	25.4	15140	11402	3739	**	17435	19570	-2135	**
Asian and other	4.9	22707	14612	8095	**	16000	16631	-632	

(continued)

Table 6.3 (continued).

Subgroup	Percent of Sample	Average Total Earnings Quarters 2-21 ($) Experi-mentals	Controls	Difference	(b)	Average Total AFDC Payments Quarters 2-21 ($) Experi-mentals	Controls	Difference	(b)
SPECIAL SUBGROUPS									
Hispanic: English speaking				(d)				(d)	
Yes	17.4	17252	13523	3729	*	17611	18883	-1272	
No	8.0	10558	6808	3751		17060	21063	-4003	***
Activity status at random assignment (c)				(d)				(d)	
In school	14.9	22149	19560	2589		16534	18329	-1796	
In any activity	15.5	21538	18903	2636		16780	18816	-2037	*
Previously in program activity	37.1	16706	14680	2027		17044	18731	-1687	**
Employed more than 20 hours per week	5.4	23682	21807	1876		12497	15858	-3361	*
Sample size (total= 3210)		1604	1606			1604	1606		

SOURCES AND NOTES: See Table 4.1.

(a) Results for a fraction of the sample reporting zero "own children" are excluded from the table.

(b) An F-test was applied to subgroup differences in "difference" estimates. Statistical significance levels are indicated as: x = 10 percent; xx = 5 percent; xxx = 1 percent.

(c) These categories are not mutually exclusive and do not include sample members with no activity status at random assignment.

(d) F-test not applied.

TABLE 6.4

SWIM

AFDC-U SUBGROUPS: IMPACTS ON EARNINGS AND AFDC PAYMENTS

Subgroup	Percent of Sample	Average Total Earnings Quarters 2-21 ($) Experi-mentals	Controls	Difference	(b)	Average Total AFDC Payments Quarters 2-21 ($) Experi-mentals	Controls	Difference	(b)
FULL SAMPLE	100.0	22878	21818	1060		19093	21054	-1961	**
IMPLEMENTATION									
SWIM office									
Service Center	50.7	24461	24046	416		19251	21263	-2011	*
San Diego West	49.3	21252	19523	1729		18930	20838	-1909	
Random assignment cohort									
July-September 1985	29.4	23569	22599	970		17696	19763	-2067	
October-December 1985	24.6	23038	18990	4048		18327	23150	-4824	***
January-March 1986	23.1	22974	19022	3952		20586	21146	-560	
April-June 1986	22.9	21607	26570	-4963		20309	20383	-74	
BARRIERS TO EMPLOYMENT									
Prior year earnings									
$3,000 or more	32.1	37973	35689	2284		15573	15933	-359	
$1-$2,999	24.5	19407	19242	165		18429	21819	-3391	**
None	43.4	13707	12988	719		22072	24393	-2321	*
High school diploma								x	
Yes	47.0	27686	24645	3041		17478	17710	-232	
No	53.0	18651	19329	-678		20559	24038	-3478	***
Number of own children (a)									
One	26.5	27573	21210	6363	**	13706	13854	-148	
Two	31.3	23512	23631	-119		17516	19319	-1803	
More than two	39.9	19336	21094	-1758		24100	27800	-3700	***

(continued)

Table 6.4 (continued).

Subgroup	Percent of Sample	Average Total Earnings Quarters 2-21 ($)				Average Total AFDC Payments Quarters 2-21 ($)			
		Experi-mentals	Controls	Difference	(b)	Experi-mentals	Controls	Difference	(b)
AFDC HISTORY AND STATUS									
Had own AFDC case									
Never	33.7	28125	27152	972		14767	14294	473	
Two years or less	34.6	23088	23301	-213		17596	20447	-2851	**
More than two years	31.8	17087	14600	2487		25242	28814	-3572	**
Welfare status								x	
Applicant	59.5	27573	25574	1999		15512	16305	-793	
Recipient	40.5	16034	16342	-308		24407	28070	-3663	***
Level of disadvantage								xx	
First-time applicants	30.6	29163	28016	1146		13842	13050	792	
Returning applicants	28.9	25861	23124	2737		17190	19625	-2435	
Less disadvantaged recipients	29.3	17740	19413	-1673		23567	25905	-2337	
More disadvantaged recipients	11.2	11795	8436	3360		26313	33681	-7369	***
ETHNICITY								xx	
White, non-Hispanic	25.6	22729	22978	-249		13176	14162	-986	
Black, non-Hispanic	20.8	23433	20276	3158		22382	22349	33	
Hispanic	40.2	25873	22703	3169		19886	21013	-1127	
Asian and other	13.4	13157	19205	-6048		22954	31962	-9008	*

(continued)

Table 6.4 (continued).

		Average Total Earnings Quarters 2-21 ($)				Average Total AFDC Payments Quarters 2-21 ($)			
Subgroup	Percent of Sample	Experi-mentals	Controls	Difference	(b)	Experi-mentals	Controls	Difference	(b)
SPECIAL SUBGROUPS									
Hispanic: English Speaking				(d)				(d)	
Yes	26.9	31971	24460	7511	**	19231	20801	-1569	
No	13.3	12352	18897	-6544		21111	21650	-539	
Activity status at random assignment (c)				(d)				(d)	
In school	8.9	21650	23844	-2194		19535	25773	-6237	**
In any activity	10.3	24321	22499	1822		19589	24624	-5036	*
Previously in program activity	29.6	20882	19702	1181		22366	26068	-3702	**
Works more than 20 hours per week	3.2	23062	31004	-7942		9141	14631	-5490	
Sample size (total= 1340)		686	654			686	654		

SOURCES AND NOTES: See Table 4.1.

(a) Results for a fraction of the sample reporting zero "own children" are excluded from the table.

(b) An F-test was applied to subgroup differences in "difference" estimates. Statistical significance levels are indicated as: x = 10 percent; xx = 5 percent; xxx = 1 percent.

(c) These categories are not mutually exclusive and do not include sample members with no activity status at random assignment.

(d) F-test not applied.

ences for implementation subgroups. Sanctioning and coverage rates were similar across offices. Across cohorts, coverage was somewhat lower for the later AFDC-FG cohorts, but not for AFDC-Us. Estimates for incremental participation and sanctioning did not differ much across cohorts.

Subgroup impacts on earnings and AFDC payments are shown in Tables 6.3 and 6.4. These tables can be used to see whether the cross-office participation differences translate into differences in impacts. For the two offices, differences in earnings impacts ran in the opposite direction from the participation differences: For both AFDC-FGs and AFDC-Us, San Diego West had larger earnings impacts than Service Center, although the differences were not statistically significant. Welfare impacts did not match the participation differences, either. Reductions in AFDC payments were a bit larger for San Diego West for AFDC-FGs and were similar across offices for AFDC-Us. Other impact estimates are not particularly revealing. Across cohorts, differences in earnings impacts and AFDC impacts were not statistically significant for AFDC-FGs or AFDC-Us, nor did the measured differences correspond to any pattern of activity indicators. However, large (or small) earnings impacts did not necessarily go with large (or small) AFDC impacts. This absence of a strong correlation between earnings impacts and AFDC impacts across subgroups will be found among other sets of subgroups below.

2. Barriers to Employment. Next, differences associated with "barriers to employment" are examined. Among the various barriers, the data at hand relate primarily to low earning power for both assistance categories, and constraints on program participation and employment facing AFDC-FGs. Low earning power is associated with (1) unfinished education, as indicated by the absence of a high school diploma or its equivalent, and (2) limited skills acquisition from work, as evidenced by a weak record of prior earnings. Number of children is used as a proxy for child care needs of AFDC-FGs.[2] Because more children increase the monthly AFDC entitlement, the "number of own children" variable also captures an increase in the relative value, for both AFDC-FGs and AFDC-Us, of remaining on AFDC compared to working.

That these several subgroups did show different propensities for work and welfare in the absence of a program intervention can be seen by studying the control mean estimates of earnings and AFDC payments in Tables 6.2 and 6.3. It is clear that prior earnings and diploma status were associated with large differences in earnings during the follow-up period for controls in both AFDC-FG and AFDC-U categories. AFDC payments also varied, although not as much. Interestingly, number of children did not have a large effect on control earnings for AFDC-FGs. It was associated with large differences in AFDC payments for both assistance categories.

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[2] However, very few AFDC-FG sample members had children under age six at baseline. Enrollment in SWIM was not mandatory for AFDC-FGs with a child under age six, except when the parent was out of the home frequently for extended periods.

For AFDC-FGs and AFDC-Us, incremental participation in "any activity" and the increase in duration of education and training were greater for experimental sample members with lesser amounts of earnings in the year prior to SWIM enrollment and for those without a diploma or its equivalent. Several of these differences were statistically significant for AFDC-FGs, but only one for AFDC-Us. However, sanctioning differences across these subgroups were not pronounced and were not statistically significant. Despite greater incremental participation, the "percent not covered" was greater for the no prior earnings and no diploma subgroups.

In general, impacts on earnings and AFDC payments differed across subgroups defined by prior earnings and diploma status. These differences in impacts were not statistically significant, but they were somewhat surprising in the light of prior research by Friedlander (1988). That research would have led to expecting below-average earnings impacts and below-average or small AFDC impacts for the more "job ready" of these subgroups. Instead, earnings impacts were generally larger for the more job ready. In particular, among these subgroups, the largest estimates of earnings impacts were for AFDC-FGs and AFDC-Us with $3,000 or more in prior-year earnings and with a high school diploma. More in line with expectations, three of these four subgroups (excepting AFDC-FGs with a diploma) had relatively low estimated AFDC payments reductions.

The pattern of earnings impacts did not fit the pattern of incremental participation, which was greater among the subgroups with less prior earnings and without a diploma. Specifically, the relatively large earnings gains for the top groups were matched by relatively low incremental participation estimates for these groups. Impacts on AFDC payments fit the subgroup participation pattern somewhat better, but only because they did not correspond to the subgroup pattern of earnings impacts: AFDC-FG welfare impacts were distributed more evenly across subgroups, while AFDC-U welfare impacts were larger for the more disadvantaged subgroups, i.e., the bottom groups. In fact, for all prior earnings and diploma subgroups below the top, AFDC impacts exceeded earnings impacts, with the excess being particularly large for AFDC-Us. These subgroup findings reinforce the office and cohort finding of a weak correlation between incremental participation measures and impacts – particularly earnings impacts – and between earnings impacts and AFDC payments impacts.

The number of children subgroups for AFDC-FG did not show a clear pattern of participation, sanctioning, or coverage differences. Earnings impacts and AFDC impacts for AFDC-FGs were largest for the one-child subgroup, although the differences were not statistically significant. Earnings impacts for the other two subgroups were not far below average, however, suggesting that barriers for mothers of school-age children were not a critical impediment to program impact. For AFDC-Us, incremental participation decreased with the number of children, and sanctioning and coverage also decreased, although neither set of differences was statistically significant. Differences in earnings impacts and AFDC impacts were large across the number of children subgroups, but the differences were not statistically significant. The pattern of earnings impacts corresponded to the pattern of incremental participation and coverage, but the pattern of AFDC impacts did not. Indeed, it is of some interest that the patterns of earnings gains and AFDC reductions were opposite: Earnings gains were largest for AFDC-Us with one child, about zero for those with two children, and somewhat negative for those with three, whereas AFDC reductions were estimated at about zero for the one-child group, about average for the two-child group, and about twice the average for the group with three or more children. This is another example of the lack of correlation between earnings gains and AFDC savings across subgroups.

3. Length of AFDC History. Length of AFDC history will be examined in two ways. First, the sample will be broken up according to "length of time on own AFDC case." Second, AFDC case status at the time of entry into the research sample will be examined. To define AFDC case status, the label "applicants" will be used for sample members who were applying for AFDC at the time of random assignment. Those who were already receiving AFDC will be called "recipients." Applicants will keep that designation throughout the analysis, even though most of them were subsequently approved for AFDC and began receiving it early in the follow-up period. Next, applicants and recipients will be further subdivided into four groups, based on length of AFDC history and other information. Applicants will be separated into first-time applicants (i.e., never had their own AFDC case before the current application) and applicants returning to AFDC after a spell off. Among recipients, the "more disadvantaged" will be defined as those with more than two years on their own AFDC case, with no earnings in the previous year, and without a high school diploma or its

equivalent. Other recipients will be labeled "less disadvantaged." "First-time applicants" largely, but not completely, overlap the "never" category under length of time on own AFDC case.[3]

Results for SWIM indicate that, as was the case with employment barriers, AFDC history has considerable predictive power on future earnings and AFDC receipt. This relationship is clearer among AFDC-Us than among AFDC-FGs. As shown by the control means in Tables 6.3 and 6.4, AFDC-U sample members with more than two years on their own case had half the earnings and twice the AFDC income over the follow-up period of those who never had their own case. Applicants had more earnings and less AFDC than recipients. The same holds for first-time applicants compared to returning applicants, and the less disadvantaged recipients compared to more disadvantaged recipients. This pattern is less clear for AFDC-FGs owing to a particular anomaly: Control earnings for those who never had their own AFDC case before and for first-time applicants appear to have been too low relative to the other subgroups. This anomaly may have been due to chance variation.

According to Tables 6.1 and 6.2, AFDC history subgroups showed differences in SWIM activity, although these differences were more evident for AFDC-FGs than for AFDC-Us. Long-term and more disadvantaged AFDC-FG recipients had larger incremental "ever participated" estimates than did the other AFDC history subgroups, and their incremental length of stay in education and training was greater. Several of these differences were statistically significant. The pattern of sanctioning was different across these AFDC-FG subgroups. Sanctioning rates did not increase smoothly across AFDC history subgroups. First-time applicants had the lowest sanctioning rate, but the highest was for returnees, and the variation was statistically significant. Percentage not covered was lowest for those with the shortest AFDC history and highest for those with the longest AFDC history and the more disadvantaged recipients, again with statistically significant patterns.

The SWIM activity patterns for AFDC-Us largely corresponded to those for AFDC-FGs, but with some differences. The incremental "ever participated" measure increased over length of AFDC history, but returning applicants had a larger-than-expected estimate. Length of stay in education and training increased over length of AFDC history and was also greater for recipients than for applicants, but the more disadvantaged recipients had a lower-than-expected estimate, which may have been associated with chance variation resulting from the small size of that subgroup (only 11 percent of the AFDC-U sample). Sanctioning rates for AFDC-Us were not notably different across AFDC history subgroups. Percentage not covered was, again, larger for the longer-term recipients and the more disadvantaged.

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[3] Some sample members listed as currently receiving AFDC at the time of random assignment (i.e., "recipients") also responded that they had never had their own AFDC case before.

As with the prior earnings and diploma subgroups, the distribution of impacts across AFDC history and status subgroups was somewhat inconsistent with prior work. Moreover, the SWIM results show dissimilarities between AFDC-FGs and AFDC-Us. On the basis of Friedlander (1988), it would be expected that sample members with short AFDC histories, especially first-time applicants, would have below-average earnings gains and below-average or small AFDC reductions. As it turned out, among AFDC-FGs, earnings gains were relatively large for the subgroups "never had own AFDC case" and first-time applicants, which largely overlap. Nor were AFDC reductions small for these subgroups, at least among AFDC-FGs. For AFDC-Us, earnings gains were about average for these subgroups, although AFDC differentials were not only small but were actually positive rather than negative.

Friedlander (1988) found the most consistent earnings gains for returning applicants. It was therefore expected that returning applicants in SWIM would show above-average earnings impacts. This was true only for AFDC-Us, although that impact was not statistically significant. AFDC-FGs in this subgroup had a slightly negative earnings effect (not statistically significant). Finally, the earlier study found evidence pointing to some difficulty for low- to moderate-cost programs in obtaining earnings gains for the "recipient" subgroup as a whole and for the "more disadvantaged" recipients in particular. But for AFDC-FGs, recipients' earnings gains were above average and statistically significant, despite that fact that earnings gains for the more disadvantaged recipients were somewhat below average. For AFDC-Us, recipients as a group did not show earnings gains, but the more disadvantaged recipients showed earnings gains above the full-sample average (not statistically significant). AFDC reductions, however, were larger than earnings gains for the more disadvantaged recipients for both AFDC-FGs and AFDC-Us. As with the earlier study, sample members who have had their own case a long time, recipients as a group, and the more disadvantaged all accounted for a significant share of SWIM's total impact on AFDC.

Among AFDC-FGs, incremental participation differences did not correlate well with differences in earnings impacts. The largest earnings impacts were for first-time applicants, who had only average and low incremental participation rates. Earnings gains were below average for the more disadvantaged recipients, who had the greatest incremental participation, both overall and in duration of education and training. Earnings gains for the less disadvantaged recipients were above average and statistically significant, and earnings gains for returning applicants were actually negative (but not statistically significant). These two subgroups had similar overall incremental participation, although the less disadvantaged recipients had an above-average incremental number of days enrolled in education or training.

At the same time, impacts on AFDC payments for AFDC-FGs were not highly correlated with earnings gains. To illustrate: For length of time on own AFDC case, earnings gains for AFDC-FGs decreased for those with the longer histories, while AFDC reductions increased. Also, returning applicants had a small *negative* earnings impact, but accrued AFDC savings only slightly below the average.

On the AFDC-U side, the distribution of earnings impacts did not closely fit the pattern of participation across subgroups, either. In particular, earnings gains for AFDC-U recipients were small, on average, even though both of their incremental participation measures were at or above average. The earnings result for recipients derives from the results for less and more disadvantaged recipients. The former had the largest increment in duration of education and training, but a negative experimental-control differential in earnings. The latter had the smallest increment in duration of education and training, but the largest earnings gain.

As was the case with the AFDC-FGs, AFDC reductions for AFDC-Us only partially corresponded with the pattern of earnings gains. In particular, AFDC-Us with more than two years on their own AFDC case and the more disadvantaged recipients both had above-average earnings gains and above-average AFDC reductions. But AFDC-Us with two years or less on their own AFDC case and less disadvantaged recipients both had negative earnings impacts and average or above-average AFDC reductions.

The four "level of disadvantage" subgroups were considered of special importance, warranting additional analysis. The combination of prior earnings and AFDC history, plus high school diploma status, has relatively strong predictive power for future earnings and AFDC. Previous research by Friedlander (1988) used a similar subgroup partition to investigate subgroup impacts in five experimental evaluations of welfare-to-work programs of the 1980s. A comparison of SWIM results with the earlier findings would therefore be useful. In addition, it was expected that SWIM participation rates would increase with the level of future AFDC receipt and would be highest among the longer-term AFDC recipients, groups three and four in this analysis. Greater participation was expected for two reasons: (1) the average length of time on the AFDC rolls after program enrollment, and, hence, the average length of time subject to the SWIM participation mandate, would be longer, and (2) the probability of being employed and therefore unable to participate would be lower. With higher participation, program operating costs would tend to be higher. It was therefore deemed important to see if costs did indeed increase with disadvantagedness and if, as a consequence of increasing costs, the returns to government budgets became negative.

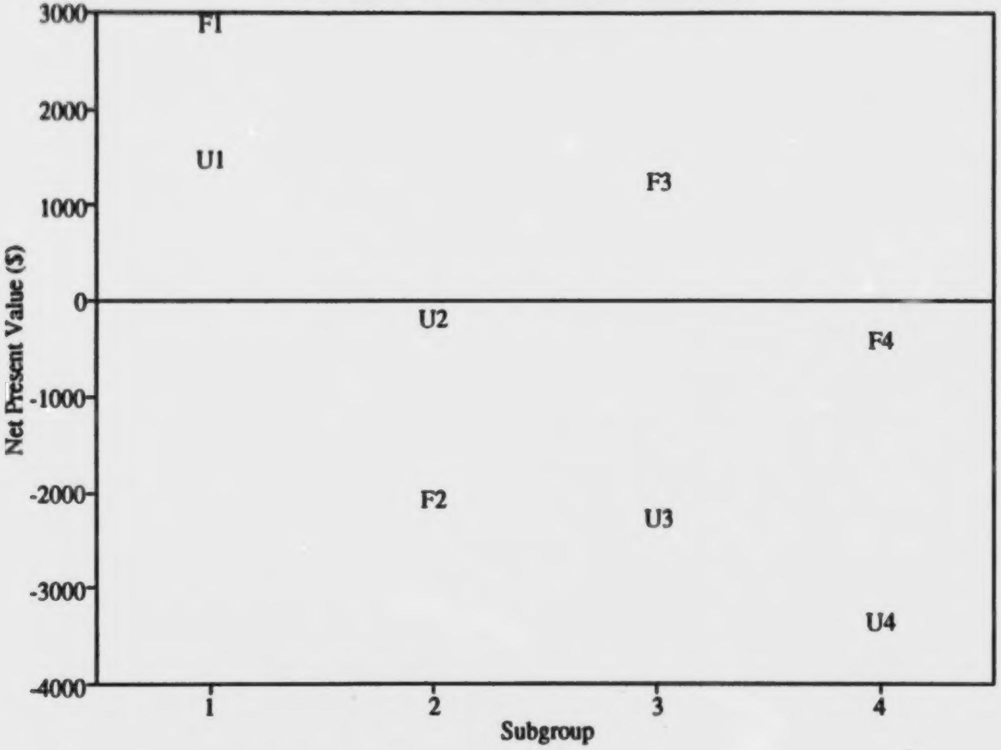
To investigate these issues further, a complete benefit-cost analysis was performed for the four prior earnings and AFDC history subgroups, both for AFDC-FGs and AFDC-Us. A description of the methodology and the detailed results of the analysis are presented in Appendix D. Here, only key aspects of those results are considered, and are presented in Figures 6.1 and 6.2. The first of these figures gives the four-way subgroup net present values from the enrollee perspective. AFDC-FGs and AFDC-Us are shown on the same graph, represented by "F" and "U," respectively; the subgroups are identified as 1, 2, 3, and 4, beginning with first-time applicants and ending with more disadvantaged recipients. Thus, for example, AFDC-FG less disadvantaged recipients are designated in the figures as group "F3."

The first graph shows net present values from the enrollee perspective for the four subgroups for AFDC-FGs and AFDC-Us. Among AFDC-FGs, there is no clear pattern of results, although it is clear that there were winners (F1 and F3) and losers (F2). Among AFDC-Us, net present value for enrollees decreased steadily from groups 1 through 4. U1s were winners and U3s and U4s were losers. This positive and negative variation in net present value may be chance variation. It is, however, consistent with the previously made observation that SWIM achieved AFDC reductions in some cases without increasing earnings to the same degree. These findings suggest that part of SWIM's effect was to bring about a change in perceptions or attitudes among some enrollees. Under SWIM, some enrollees may have come to view continued welfare receipt in a less favorable light; some may have come to see SWIM as increasing the "hassle" of remaining on AFDC. These enrollees might therefore have been induced to leave AFDC sooner than they would have otherwise and without necessarily receiving higher earnings than otherwise – possibly without any observable earnings of their own if they could obtain income from another family member. The evidence suggests that this kind of deterrence may have been one of the underlying mechanisms of SWIM's impact.

FIGURE 6.1

SWIM

ALL AFDC-FG AND ALL AFDC-U:
NET GAINS TO ENROLLEES BY SUBGROUP



SOURCE: Appendix Tables D.2, D.3, D.5, D.6, D.8, D.9, D.11, D.12.

KEY:

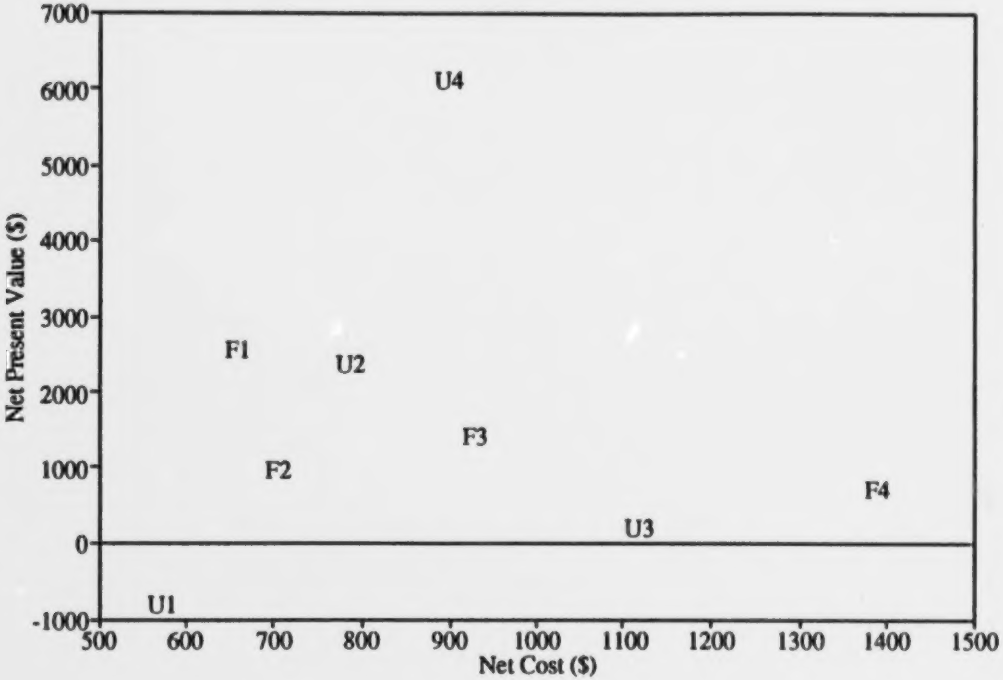
F=AFDC-FG; U=AFDC-U.

1=First-time applicants; 2=Returning applicants; 3=Less disadvantaged recipients; 4=More disadvantaged recipients.

FIGURE 6.2

SWIM

ALL AFDC-FG AND ALL AFDC-U: NET GAINS TO GOVERNMENT BUDGETS BY SUBGROUP NET COST



SOURCE: Appendix Tables D.2, D.3, D.5, D.6, D.8, D.9, D.11, D.12.

KEY:

F=AFDC-FG; U=AFDC-U.

1=First-time applicants; 2=Returning applicants; 3=Less disadvantaged recipients; 4=More disadvantaged recipients.

The second figure plots the net present values from the government budget perspective. Instead of plotting these by subgroup name, the figure plots them by the net cost of working with each of the four subgroups. This figure indicates, first, that net costs did, as expected, increase with the level of disadvantagedness. Progressing from subgroup 1 to 2, 3, and then 4 yields increased net costs consistently for AFDC-FGs and nearly consistently for AFDC-Us. The only exception to the pattern is the reversal between U3 and U4. These net cost differences are not trivial. For example, net costs of the F4 subgroup were more that twice those of the F1 subgroup. And there may be other ways of splitting up the samples that would show even larger differences.

Second, the greater net cost of the more disadvantaged subgroups does not mean that the program was unable to break even for them. In fact – and this point is critical – the program broke even for all subgroups for AFDC-FGs and AFDC-Us except for first-time AFDC-U applicants. Subgroups F4 and U3, with the greatest net costs, did have relatively low net present values, but they were not negative. From this array of government budget results, it can be concluded (1) that increased disadvantagedness, at least as defined from these objective characteristics, was associated with increased net cost, but (2) the higher net cost did not prohibit the SWIM program from breaking even for government budgets. It may be, however, that in other kinds of programs, higher costs for the more disadvantaged could more than offset government budget savings achieved through welfare reductions. The issue may merit additional field research in other settings.

These findings in part confirm and in part differ from those of the earlier subgroup analyses of broad-coverage programs by Friedlander (1988). In that study, impacts on earnings and AFDC were analyzed for three mutually exclusive sets of subgroups or "tiers" of increasing disadvantagedness for four random assignment experiments. Tier one was first-time applicants; tier two, returning applicants; tier three, all recipients. The study also looked at impacts for several other subgroups defined by background characteristics. The study found impacts for a variety of subgroups across the four programs examined. That analysis concluded that evidence for exclusively targeting any specific subgroup, whether the least or most disadvantaged, was lacking; highlighted the important contribution of the recipient subgroups to total AFDC savings; and pointed out the apparent weakness of estimated earnings gains for the most disadvantaged.

These conclusions are largely borne out in the SWIM data. Not borne out was a conclusion concerning the relative magnitude of earnings gains for the two applicant subgroups. The earlier study found that earnings gains for AFDC-FGs in four programs were small for first-time applicants and appeared most consistently for returning applicants. The opposite was found among AFDC-FGs in SWIM. This reversal may have resulted from a statistical anomaly – the low ratio of control earnings for AFDC-FG first-time applicants and returning applicants (mentioned above) – or from some distinctive feature of SWIM; or it may indicate a deficiency in the original hypothesis that earnings gains will not necessarily be largest among the "most employable." In choosing among these three possible explanations, it is worth noting that the earlier study's pattern of above-average earnings gains for returning applicants was found for AFDC-Us in SWIM, which might suggest that the AFDC-FG results were a statistical anomaly. Also supporting this explanation is the fact that, looking downward on the AFDC-FG table from first-time applicants to returnees to less and then more disadvantaged recipients, earnings gains do not move uniformly in one direction. That is, the earnings impact estimates go down, then up, then down again, which suggests some degree of chance variation.

On this particular issue, namely, the relative magnitude of earnings gains for first-time applicants and returning applicants, a definitive judgment cannot be reached from the SWIM data. More important is the finding of earnings gains and AFDC reductions for a number of subgroups across the spectrum of prior earnings and AFDC history. These findings, together with the finding that the four subgroups almost always reached the government budget break-even point or better, support the basic rationale for a broad rather than narrow targeting strategy, particularly when AFDC reductions are an important program goal. From the enrollee perspective, the SWIM results are consistent with those of the previous study regarding the high ratio of AFDC reductions to earnings gains among the most disadvantaged.

4. Ethnicity. Non-Hispanic white, non-Hispanic black, Hispanic, and Asian/other subgroups were distributed differently across AFDC-FG and AFDC-U assistance categories. The largest ethnic group for AFDC-FGs was non-Hispanic blacks; the largest for AFDC-Us was Hispanics. In addition, AFDC-Us had a significant percentage of Asians. The Asian/other ethnic category for AFDC-FGs was too small to produce precise estimates.[4]

For non-Hispanic whites, behavior was similar for AFDC-FGs and AFDC-Us. Non-Hispanic whites had below-average incremental participation rates and relatively low sanctioning rates. They nevertheless had about-average coverage rates, resulting, in part, from the relatively low propensity of non-Hispanic whites (both experimentals and controls) to remain on AFDC during the follow-up period. Impacts on AFDC payments (statistically significant) were larger than earnings impacts (not statistically significant) for AFDC-FGs; the same was true for AFDC-Us, with earnings impacts near zero.

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[4] Within the Asian/other ethnic category, 75 percent of the AFDC-FGs and 85 percent of the AFDC-Us were Asians or Pacific Islanders.

Non-Hispanic blacks in both the AFDC-FG and AFDC-U assistance categories had incremental participation that was generally about average and was somewhat greater than that of non-Hispanic whites. Sanctioning rates, however, were much higher for non-Hispanic blacks, about double the rate for non-Hispanic whites and Hispanics. Percentage not covered was about average for non-Hispanic blacks, indicating that their higher sanctioning rates were not reflective of longer stays on welfare, as compared to non-Hispanic whites or Hispanics. The reason for the high sanctioning rates among non-Hispanic blacks is unclear. Non-Hispanic black AFDC-FGs and AFDC-Us showed opposite impact results: no statistically significant earnings impacts but about-average and statistically significant AFDC impacts for AFDC-FGs; above-average earnings impacts and no AFDC impacts (neither statistically significant) for AFDC-Us.

SWIM planners made an effort to provide Spanish-language job search assistance and work experience positions, and English as a Second Language courses were assigned for many Hispanics as an education activity.[5] Incremental "ever participated" estimates for Hispanic AFDC-FGs and AFDC-Us were similar to rates for other groups. Their incremental length of stay in education and training was substantially greater than that of non-Hispanic whites and blacks in both the AFDC-FG and AFDC-U assistance categories, and Hispanics, who comprised 25 percent of the AFDC-FG sample and 40 percent of the AFDC-U sample, accounted for more than half the total program increment to education and training time. Sanctioning of Hispanics was below average for AFDC-FGs and AFDC-Us; coverage was close to the sample average. Earnings impacts for Hispanics in both assistance categories were above average ($3,739 for AFDC-FGs and $3,169 for

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[5] SWIM staff expressed enthusiasm about job search workshops conducted in Spanish and unpaid work experience positions that could accommodate individuals monolingual in Spanish, both of which were components that had not been previously implemented in San Diego. Staff felt that the workshops would be particularly helpful for Spanish-speaking participants because many of these individuals were perceived to be unfamiliar with the job-seeking process and mores of the United States. The first week of the Spanish-language workshop consisted of group sessions focusing on how to write resumes, locate job leads, handle an interview, and use the telephone to obtain appointments. During the second week, instead of placing calls to prospective employers, which was the activity in the English-speaking workshop, Spanish-speaking participants were required to make three in-person employer contacts per day. Their limitations in English made blind calls to prospective employers impractical.

AFDC-Us), and AFDC reductions were above average for AFDC-FGs and somewhat below average for AFDC-Us. These Hispanic earnings and AFDC impacts were statistically significant for AFDC-FGs but not for AFDC-Us. Of particular interest is the pattern of Hispanic earnings gains over time. As shown in Appendix Tables B.3 and C.3, Hispanic earnings gains in year five were not much below those of year two, indicating a relatively stable pattern over time, with additional impacts possibly accruing after year five. These findings are noteworthy in that impacts for Hispanics in employment and training programs generally have been small and not statistically significant in the few studies that have included this ethnic group. Moreover, these studies have indicated that such programs are usually more effective for other ethnic groups.[6]

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[6] Impacts for ethnicity subgroups have been estimated based on experimental designs in several recent studies. All of these studies have included Hispanic AFDC recipients, although two of the studies included individuals not receiving welfare as well.

In a study of subgroup impacts for selected welfare employment programs operated in the 1980s, Friedlander (1988) found small *negative* average quarterly earnings impacts and small *positive* average quarterly AFDC impacts for Hispanic applicants for AFDC in welfare employment programs in San Diego and Chicago, and small average quarterly earnings gains and no average quarterly AFDC impacts for Hispanic AFDC recipients in the Chicago program (recipients were not included in the San Diego program). None of these impacts were statistically significant.

In the multi-site Minority Female Single Parent Demonstration, education, training, and support services were provided to volunteer minority single mothers of any age, using program models that varied by site (Gordon and Burghardt, 1990, and Burghardt et al., 1992). One of the four sites had a large number of Hispanics. In this site, located in the San Jose, California, area, impacts on the average monthly employment rate during the 12 months following random assignment to a control or experimental group were larger for blacks than for Hispanics, although this difference was not statistically significant. Hispanics at this site did have positive impacts, although they were not statistically significant. More recent findings, which cover a 30-month follow-up period and include more outcome measures, show a slightly different picture. Impacts on the average monthly employment rate during the last year of the 30-month follow-up period remained positive (but not statistically significant) and became slightly *higher* for Hispanics than for blacks, although this difference was again not statistically significant. Hispanics also had earnings impacts (statistically significant) during the last year of follow-up that were similar to those of blacks. During this same period, Hispanics also experienced larger (not statistically significant) reductions in average monthly rates of AFDC receipt, compared to those estimated for blacks, but this difference was not statistically significant.

In the National JTPA Study, 18-month earnings impacts for Hispanic adult women were actually negative, although not statistically significant (Bloom et al., 1993). These impacts were lower than those for other ethnic groups, with the differences being statistically significant at the 80 percent level. However, after controlling for differences in the distributions of the three ethnic groups across the study sites, estimated impacts were not significantly different from one another. Similar results were found for Hispanic adult men. Their 18-month earnings impacts were lower than those of the other ethnic groups, but neither their estimated impact nor the differences in impacts across ethnic groups were statistically significant.

Unpublished findings from an MDRC study of the GAIN program in California indicate that in the six counties included in the evaluation, Hispanic sample members heading single-parent (AFDC-FG) cases experienced small, not statistically significant earnings impacts in the first year of follow-up and small but

(continued...)

In order to determine whether Hispanic impacts might be associated with English as a Second Language assignments for monolingual Spanish speakers, the Hispanic group was subdivided into monolingual Spanish and nonmonolingual (i.e., English speakers), and separate subgroup estimates were produced for each. These estimates are shown at the bottom of the subgroup tables. In examining them, there are two primary questions: Did non-English speakers receive a significant increment of education and training? And did they experience impacts from SWIM?

As shown in the table, non-English speakers did receive incremental education and training time above the full-sample average for both AFDC-FGs and AFDC-Us, but only for AFDC-Us was this increment quite large. This large incremental treatment was not, however, associated with a positive earnings impact. In fact, the earnings impact for the AFDC-U Hispanic monolingual subgroup was negative (not statistically significant) and the AFDC impact was small (and not statistically significant). Among AFDC-FGs, earnings impacts and AFDC impacts were both large (with only the latter being statistically significant), but the increment in education and training was not as large as for the English-speaking Hispanic subgroup. These mixed results cannot confirm either that impacts were obtained consistently for non-English-speaking Hispanics or that a strong English as a Second Language emphasis was important for them. It is worth noting, however, that without special assistance, Hispanic controls who were non-English speaking produced comparatively low earnings over the follow-up period, and among AFDC-FGs, they received somewhat more in AFDC payments.

Sanctioning rates were quite low for the Asian/other ethnic group for both AFDC-FGs and AFDC-Us. In other respects, however, this subgroup behaved differently across AFDC-FG and AFDC-U categories. Among AFDC-FGs, the subgroup had about an average incremental "ever participated" rate but quite a large increment to education and training time. The same subgroup also had a large earnings impact, but only a small reduction in AFDC payments. AFDC-Us, however, had relatively low incremental overall and education/training participation, a large negative earnings impact, and a large AFDC payments reduction.

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[6](...continued)
statistically significant impacts on AFDC payments during the same follow-up period. Although statistical tests of the differences in impacts across ethnic groups have not yet been performed, the impacts on both outcomes for Hispanics were lower than those obtained for the other ethnic groups. Hispanic sample members heading two-parent (AFDC-U) cases experienced earnings impacts (not statistically significant) and statistically significant AFDC payments impacts in the first follow-up year. These impacts were smaller than those of non-Hispanic whites and blacks but larger than those of Asians although, again, statistical tests of the differences in impacts across ethnic groups have not yet been calculated.

To determine whether the array of impacts calculated for the various ethnic groups reflects inter-group differences in background characteristics, "conditional" estimates were prepared of the subgroup experimental-control differences.[7] These conditional estimates statistically remove the effects of background demographic differences and differences in local office residence from the ethnic subgroup estimates. For example, if differences in impacts across ethnic subgroups were mainly a result of ethnic differences in prior earnings and AFDC history, then conditional estimates that account for those characteristics will be closer to each other than the estimates that have just been examined.

The first question of the conditional estimates is whether the above-average sanctioning rates for non-Hispanic blacks can be explained by a correlation of sanctioning with other demographic characteristics of the non-Hispanic black subgroup. The conditional estimates indicate that they were not. Unconditional differences in sanctioning between non-Hispanic whites and non-Hispanic blacks were 7.4 percentage points for AFDC-FGs and 7.2 percentage points for AFDC-Us, compared to conditional estimates of 7.5 percentage points and 7.8 percentage points, respectively.

Next, conditional impact estimates are examined for the ethnic subgroups, as shown in Figures 6.3 and 6.4[8] These figures plot the conditional estimate for each subgroup (vertical axis) against the unconditional estimate for the same subgroup (horizontal axis). The first plot shows earnings impacts; the second shows AFDC impacts with the sign reversed so that AFDC reductions appear as positive amounts. The diagonal line plotted in each figure indicates equality between unconditional and conditional estimates: For points above the line, the conditional estimate exceeded the unconditional estimate; for points below the line, the conditional estimate was less than the unconditional estimate. Again, the convention is used of identifying AFDC-FG points with the prefix "F" and AFDC-U points with the prefix "U." The subgroups are labeled "W" for non-Hispanic white, "B" for non-Hispanic black, "H" for Hispanic, and "A" for Asian/other.

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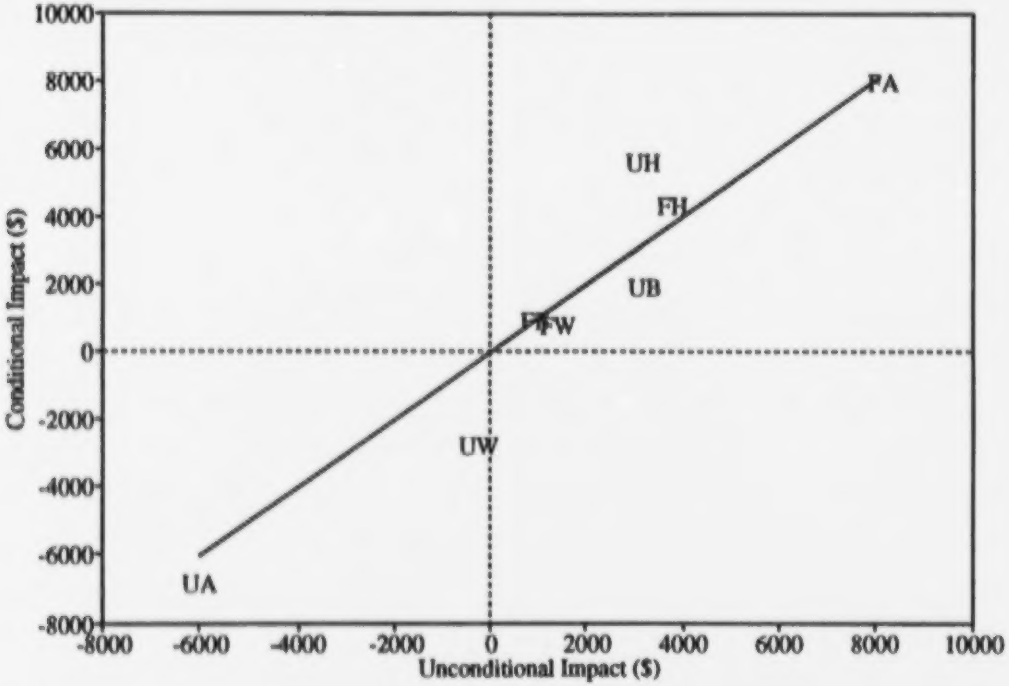
[7] Conditional impacts were produced from the coefficients of the interaction terms of the regression treatment dummy variable after adding interactions between the treatment dummy and SWIM office, random assignment cohort, prior-year earnings, high school diploma, number of children, length of time had own AFDC case, and applicant/recipient status. Conditional estimates for each outcome variable were constrained so that their weighted mean would equal the full-sample impact for that variable, with weights set to the fraction of the AFDC-FG or AFDC-U sample in each ethnic category.

[8] Conditional earnings/AFDC impact estimates for AFDC-FGs were $842/-$2,069 for non-Hispanic whites, $944/-$1,637 for non-Hispanic blacks, $4,334/-$2,354 for Hispanics, and $7,999/-$672 for Asians and others; and for AFDC-Us were -$2,733/-$2,203 for non-Hispanic whites, $1,956/-$553 for non-Hispanic blacks, $5,638/-$141 for Hispanics, and -$6,841/-$8,723 for Asians and others.

FIGURE 6.3

SWIM

ALL AFDC-FG AND ALL AFDC-U: UNCONDITIONAL BY CONDITIONAL EARNINGS IMPACTS FOR ETHNIC SUBGROUPS



SOURCE: Tables 6.3 and 6.4 and computations by MDRC.

KEY:

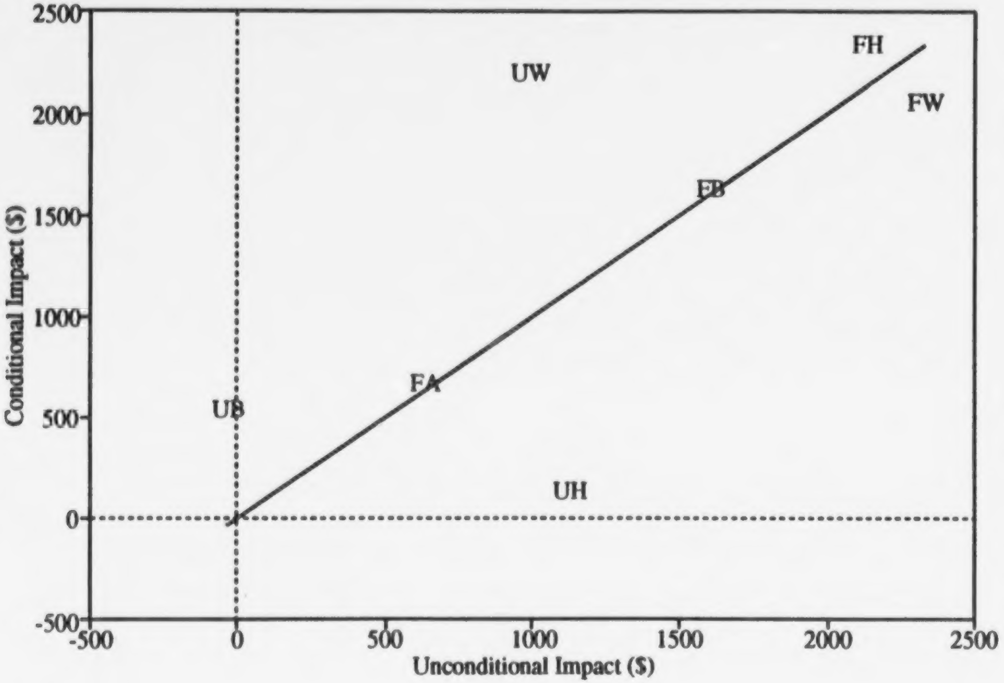
F=AFDC-FG; U=AFDC-U.

W=White, non-Hispanic; B=Black, non-Hispanic; H=Hispanic; A=Asian and other.

FIGURE 6.4

SWIM

ALL AFDC-FG AND ALL AFDC-U:
UNCONDITIONAL BY CONDITIONAL AFDC PAYMENTS IMPACTS FOR ETHNIC SUBGROUPS (a)



SOURCE: Tables 6.3 and 6.4 and computations by MDRC.
(a) "UA" subgroup not shown.

KEY:
F=AFDC-FG; U=AFDC-U.
W=White, non-Hispanic; B=Black, non-Hispanic; H=Hispanic; A=Asian and other.

The primary question concerns the earnings figure: Was the somewhat below-average earnings impact for AFDC-FG non-Hispanic blacks associated with the demographics of that subgroup? It was not. The conditional earnings impact was almost identical to the unconditional. One can also see that the below-average earnings gains for non-Hispanic whites for both AFDC-FGs and AFDC-Us were not the result of demographic differences, since the conditional estimates were even lower.

The AFDC figure excludes the AFDC-U Asian/other subgroup, which would be located too far up and to the right to preserve definition in the display. Unconditional and conditional estimates for that subgroup were similar. The differences shown in the figure are, for the most part, fairly modest. The largest changes are for AFDC-U non-Hispanic whites, for whom conditional AFDC reductions increased, and AFDC-U Hispanics, for whom AFDC reductions decreased. It is not clear why these changes should occur, especially since conditional earnings impacts for these same subgroups moved in the opposite direction (i.e., earnings impacts decreased for AFDC-U non-Hispanic whites and increased for AFDC-U Hispanics). This is another aspect of the weak correlation between impacts on earnings and AFDC payments.

As with the other subgroups, findings for ethnicity must be interpreted with care. Certain conclusions do appear warranted, however. First, it seems clear that Hispanics were not underserved relative to non-Hispanic whites and blacks. Their impact estimates for both earnings and AFDC payments also indicate that SWIM was successful in working with them, although it is not clear which components of SWIM produced these impacts.[9] Second, the high sanctioning rate for blacks for both AFDC-FGs and AFDC-Us is worthy of note, although the reason for the heavy sanctioning is not evident, and the heavy sanctioning did not translate into greater-than-average impacts on AFDC income. This evidence supports the earlier tentative conclusion that the large AFDC savings produced by SWIM were not the direct result of grant reductions produced by sanctions. Third, the ethnicity subgroup results offer further examples of the weak correlation between the magnitude of the earnings impact for a subgroup and the magnitude of its AFDC payments impact.

5. **Current or Prior Activity Status**. The final subgroup dimension to be examined concerns activities at the time of random assignment. A significant minority of enrollees were already involved or had recently been involved in some employment-directed activity, were in school, or were already

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[9] In fact, there is not a clear correlation between large increments in education and training time and higher earnings impacts within other subgroup dimensions, either.

employed, as of random assignment. For individuals already active, the aim is to see whether the SWIM program produced any additional effects, especially since those already engaged in qualifying education and training could be excused from the regular SWIM sequence of activities for as long as they remained active. For former participants who were still on AFDC, the question is whether further efforts by the program could produce impacts. Enrollees already employed more than 20 hours a week did not have to participate in SWIM as long as they remained employed, so it is of interest to compare the subsequent earnings and AFDC income for experimentals and controls in this subgroup. Estimates for the four activity subgroups are presented in the last panel of the tables. Membership in these subgroups is not mutually exclusive; some sample members may have belonged to more than one group.

The first two subgroups, labeled "in school" and "in any activity" in Tables 6.1 through 6.4, are often referred to as "self-initiated participants," i.e., individuals who have already sought out education or training programs, on their own initiative, prior to enrolling in welfare-to-work programs. Programs differ in their treatment of these individuals. In SWIM, self-initiated participants' activities were reviewed by case managers when they attended the program orientation. If their activities met with SWIM's approval criteria, these individuals would be excused from the regular sequence of SWIM components (job search followed by unpaid work experience) and allowed to continue their self-initiated activities. If their activities did not meet the approval criteria, these individuals would be assigned to the regular SWIM components. Results presented in Hamilton (1988) indicate that approximately half of the individuals who reported being currently in school or another activity (usually training) as of random assignment had their activities "approved" by SWIM. SWIM staff closely monitored, every 30 to 45 days, the attendance of the approved self-initiated participants. If these individuals completed or dropped out of their programs, they would be subsequently assigned to the regular SWIM components.

In SWIM, the difference between the treatment of experimentals and controls in the "in school" and "in any activity" subgroups thus consisted of the following: First, experimentals would have heard SWIM's "message" emphasizing work over welfare in their initial program orientation and in their contact with case workers; controls would not have heard such messages. Second, experimentals whose programs were not approved by SWIM would have been assigned to job search and unpaid work experience, possibly discouraging them from continuing with their self-initiated education or training program, whereas controls would have been given no restrictions or advice regarding their self-initiated programs and no other assignments. Finally, experimentals' attendance in their programs would have been closely monitored, most commonly through verification forms that sample members would have had to submit to their school and return to their caseworkers, whereas control group members would not have undergone any scrutiny through SWIM. It is important to note that SWIM did not provide child care assistance to any self-initiated participants, regardless of whether they were experimentals or controls. Consequently, experimental-control differences for these subgroups in participation or impacts cannot be interpreted as representing the effects of providing child care assistance.

The results indicate that AFDC-FG and AFDC-U controls who were in school or in any activity at the time of SWIM enrollment had relatively high rates of participation during follow-up for any activity and for education and training in particular. As might be expected, then, the incremental activity estimates for those two subgroups were relatively low. Impact estimates for these subgroups were not low, however, with the exception of earnings impacts for AFDC-Us in school.

Individuals active previously make up a significant share of the samples, about one-third for both AFDC-FGs and AFDC-Us. Their incremental activity estimates were not notably low. They were almost at the full-sample average for any activity for AFDC-FGs and AFDC-Us, and for education or training were above the full-sample average for AFDC-FGs and somewhat below the full-sample average for AFDC-Us. Again, the impact estimates were not particularly low, either. These results suggest that SWIM produced an effect even for those who had recently been involved in employment-directed activities as a result of their own initiative or a welfare-to-work program.

For sample members employed more than 20 hours per week at the time of random assignment, incremental participation rates were considerably below average for any activity for both AFDC-FGs and AFDC-Us and for education or training for AFDC-Us. This resulted from relatively low activity estimates for the experimental groups, which would be expected for individuals already at work. Impacts were not low for AFDC-FGs in this subgroup. For AFDC-Us, earnings gains were low – in fact, they were negative for this small subgroup – but AFDC reductions were not small.

In sum, while incremental activity among those currently active or working as of program enrollment was low, there does not appear to be a pattern of consistently low impacts. It is not clear why this should be. It may be that rather modest increments in activity can produce effects for these groups for some reason. Alternatively, the findings may mean that other elements of SWIM besides participation in formal activities played an important role in producing effects for these subgroups and possibly for other enrollees. The other elements may have included the SWIM "message" emphasizing work over welfare, which may have led to changes in attitudes or perceptions, SWIM's case management, the close monitoring of experimentals' activities, or the requirement that individuals participate in job search and unpaid work experience if they dropped out of their self-initiated programs. Hard-and-fast conclusions are not possible, however, especially given the small size of most of these subgroups.

6. Mechanism of Effect. At the start of this report, six possible underlying mechanisms were listed that might, to different degrees, account for the SWIM program impacts: increased earning power, removal of barriers to employment, increased work motivation, increased relative cost of AFDC receipt, earnings discovery, and sanctioning. Now, at the end of the empirical analysis, it is appropriate to marshall the evidence for and against each of these mechanisms.

It is easiest to deal first with the earnings discovery and sanctioning hypotheses. Had earnings discovery been a major component of SWIM impacts, one would have expected to see a decrease in the fraction of experimentals who were concurrently employed and on AFDC (provided, of course, that the discovered earnings were reported to the Unemployment Insurance system). Although this report's measures of employment and AFDC status are quarterly rather than monthly, and therefore allow some slack in the overlap between earnings and AFDC payments, they nevertheless do not show the required effect.[10] Direct AFDC reductions through sanctioning also appear to have been a small part of the overall SWIM effect. For AFDC-FGs, sanctions only reduced payments; they did not close cases. But the great bulk of AFDC reductions for AFDC-FGs were achieved through reductions in months rather than in monthly grant amounts. For AFDC-Us, the pattern of months versus monthly amounts is less relevant, since sanctions for them could have closed cases. But subgroups with substantially above-average sanctioning rates, both for AFDC-FGs and AFDC-Us, did not show correspondingly large AFDC reductions. These results do not rule out possible indirect effects from the threat of a sanction, whether as a tool in securing compliance for some enrollees or in increasing the perceived "hassle" of remaining on AFDC for others.

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[10] It is possible that discovered earnings would reduce the AFDC grant amount without resulting in complete case closure. For AFDC-FGs, however, reductions in AFDC payments came mostly through fewer months on the rolls rather than lower monthly payments for those remaining on welfare. This argues against discovery and partial reduction. Lower monthly payment amounts were more common among AFDC-Us. It is conceivable that earnings discovery with partial grant reduction did play a role for AFDC-Us.

Increased earning power, if it were the dominant mechanism, would have tended to produce earnings per employed quarter that were greater for experimentals than controls. Instead, that measure was calculated to be similar for experimentals and controls among AFDC-FGs and actually lower for experimentals among AFDC-Us. Only if earning power increases occurred mainly for persons who originally had quite low earning power would this pattern be consistent with the earning power hypothesis. The failure of the experimental-control earnings differential to persist or grow, coupled with the absence of long-term increases in the fraction of experimentals in the higher earnings brackets, argues against increased earning power. The strong correlation between incremental participation in education and training and earnings impact, which one would expect if increased earning power through skills-building were a major component of impact, appears instead to be weak. This may, however, merely mean that greater effort is required to achieve earnings impacts with the more disadvantaged, who experienced above-average increases in education and training. The increased earning power hypothesis also leads to the expectation of a positive correlation between earnings impacts and AFDC impacts. The correlation is weak, but this may have resulted from the above-average earnings gains to some less disadvantaged subgroups, for whom AFDC reductions are limited by their relatively short future length of stay on AFDC in the absence of special services. It is important to be aware of the ambiguities in this body of evidence. Yet the weight of evidence clearly is not strongly consistent with increased earning power being the main mechanism of the SWIM program effect.

That leaves barrier removal, increased work motivation, and increased relative cost of AFDC receipt. These three mechanisms are the most difficult to distinguish empirically. In fact, barrier removal and increased work motivation lead to quite similar predictions about the behavior one should observe with the evaluation data.[11] Both are consistent with the finding of increased earnings being explained mostly by increased employment. Barrier removal would imply that earnings gains should be greater than AFDC reductions, whereas increased work motivation could lead to the latter exceeding the former. But both of these mechanisms see increased exit from AFDC only as a response to increase job-finding, implying a high correlation between earnings impact and AFDC impact across subgroups. The actual correlation appears to be weak for both AFDC-FGs and AFDC-Us. In addition, the ratio of AFDC reductions to earnings gains is often relatively high: near unity for several subgroups and greater than unity for some. The net present value of five-year earnings gains minus AFDC reductions was near zero or negative for five of the eight subgroups in the subgroup benefit-cost analysis.

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[11] Data about use of child care, transportation, and other barriers and about motivation and attitudes among experimentals and controls would be helpful in addressing these hypotheses.

These empirical patterns suggest that part of the SWIM program effect may have resulted from the increased relative cost mechanism. The increase in "not employed and off AFDC" observed for AFDC-FGs, which reached statistical significance in year two, is also consistent with an increased relative cost of AFDC receipt compared to the alternatives. Such an empirical increase in non-work and non-welfare is clearly not consistent with barrier removal or increased work motivation. It would, however, be consistent with earnings discovery if the discovered earnings were mostly off-the-books earnings that were not Unemployment Insurance-reported or were earnings not covered by Unemployment Insurance. The relative cost mechanism would also account for the weak correlation across subgroups between incremental participation and impact, and between earnings gains and AFDC reductions. Although certainty is not possible, these findings support increased costs of maintaining the AFDC grant as one of the mechanisms of the SWIM program impact.

Given the kinds of data available, it is not possible to conclude precisely how much each mechanism contributed to the overall SWIM impact. All may have contributed something. Nor can one conclude with great confidence that one or another mechanism was the dominant one. Some of the conclusions are based on nonexperimental comparisons. It may also be possible to put forward alternative mechanisms to explain the observed pattern of results. For example, earning power may have increased among some enrollees, only to be offset by other enrollees finding jobs with below-average pay. Finally, the absence of information for each sample member about specific barriers, motivations, attitudes, and other details limits the ability to corroborate conclusions about the roles these factors might play.

APPENDICES

APPENDIX A

RESULTS OF GAIN CASEFILE REVIEWS

Tables A.1 through A.3 present an analysis of SWIM and GAIN participation for a random subsample of 401 experimentals and controls from the SWIM impact sample (271 AFDC-FGs and 130 AFDC-Us). Information about SWIM activity came from the data sources utilized for the main activity analysis in the SWIM evaluation. Information about GAIN activity came from a search of GAIN casefile records.

Table A.1 indicates that a substantial proportion of SWIM control group members did receive GAIN services. Approximately 31 percent of the AFDC-FG controls and 27 percent of the AFDC-U controls attended a GAIN orientation, in which they would have been told about the services available under GAIN and their obligation to participate. Approximately 20 percent of the AFDC-FG and AFDC-U controls participated in GAIN-operated job search, education, training, work experience (PREP), or GAIN-approved client-initiated education or training. Some control group members were also involved in the sanctioning process owing to noncompliance with GAIN participation requirements, although only a small proportion appear to have left GAIN because GAIN staff requested that the welfare department actually reduce their AFDC grants (referred to as "deregistration due to sanctioning" in Table A.1). These activity statistics indicate that a sizable proportion of the SWIM control group members experienced the key elements of a welfare-to-work program treatment – namely, program services and the program participation requirement – at some point during the five-year SWIM follow-up period. These statistics seem particularly high in light of the percentage of control group members (58.9 percent of the AFDC-FGs and 54.8 percent of the AFDC-Us) who were receiving AFDC in the third year of the follow-up period, the approximate point in time when control group members became eligible for GAIN.

Comparing the GAIN participation rates for controls and experimentals, Table A.1 indicates that incremental participation in GAIN was small for AFDC-FGs. Similar proportions of experimentals and controls in the SWIM impact sample ever participated in GAIN job search, education, training, work experience (PREP), or client-initiated education or training, although control group participation rates were generally slightly higher than those of the experimental group. A slightly higher proportion of controls appears to have been deregistered from GAIN owing to sanctioning as well.

TABLE A.1

SWIM-GAIN CASEFILE STUDY

GAIN ACTIVITY ESTIMATES BY ASSISTANCE CATEGORY AND RESEARCH GROUP

	AFDC-FG			AFDC-U		
Activity Measure	Experimentals	Controls	Difference	Experimentals	Controls	Difference
Attended GAIN orientation (%)	30.4	30.9	-0.5	14.1	27.3	-13.2 *
Participated in any GAIN activity (%) (job search, PREP, education, training, or self-initiated education or training)	17.6	19.5	-1.9	7.8	19.7	-11.9 **
Participated in job search (%)	9.5	12.2	-2.7	4.7	18.2	-13.5 **
Participated in PREP (%)	0.0	0.8	-0.8	0.0	3.0	-3.0
Participated in education (%)	6.8	7.3	-0.5	4.7	10.6	-5.9
Participated in training (%)	2.0	3.3	-1.3	1.6	3.0	-1.4
Participated in GAIN-approved self-initiated education or training (%)	6.1	4.1	2.0	3.1	3.0	0.1
Referred to money management (%)	1.4	4.1	-2.7	0.0	1.5	-1.5
Placed in money management (%)	0.0	0.0	0.0	0.0	0.0	0.0
Referred for sanction (%)	4.1	4.9	-0.8	1.6	3.0	-1.4
Deregistered due to sanction (%)	1.4	4.1	-2.7	1.6	0.0	1.6
Sample size (AFDC-FG total = 271) (AFDC-U total = 130)	148	123		64	66	

SOURCE: MDRC calculations from GAIN casefile records maintained by the County of San Diego Department of Social Services.

NOTES: The sample for this table consists of a subsample of individuals who registered with SWIM between July 1985 and June 1986.

Activity measures are calculated as a percent of the total number of persons in the indicated assistance category and research group. Follow-up begins with the date of initial SWIM registration and ends five years later.

Participation is defined as attending a GAIN job search activity, PREP (GAIN work experience), GAIN-referred education or training, or GAIN-approved client-initiated education or training for at least one day.

A two-tailed t-test was applied to differences between experimental and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

Experimental-control differences were larger among the AFDC-Us, although precise estimates are not possible, given the small size of the AFDC-U subsample. The participation rate among AFDC-U controls for any GAIN activity was about 12 percentage points higher than the rate among AFDC-U experimentals. The bulk of this difference appears to be associated with job search, but AFDC-U controls also participated more than experimentals in GAIN work experience and education and, to a lesser extent, in training.

Appendix Table A.2 indicates that most of the GAIN participation observed for the SWIM experimentals and controls occurred during years three through five of the follow-up period. Large experimental-control differences in any GAIN participation among the AFDC-Us, however, did not occur until years four and five.

Data presented in Appendix Table A.3 indicate that over four-fifths of the AFDC-FG and AFDC-U experimentals who participated in GAIN activities had participated in an employment-directed activity earlier in the follow-up period, either as a result of a SWIM assignment or of self-initiated enrollment, prior to the start of GAIN. This was not the case for AFDC-FG and AFDC-U controls: Only about one-third of the controls who participated in GAIN had participated in an employment-directed activity (as a result of self-initiated enrollment) prior to the start of GAIN. This suggests that among AFDC-FG and AFDC-U controls (but not among their counterparts in the experimental group), GAIN engaged individuals who had not previously been exposed to welfare-to-work program mandates or employment-directed services.

Combining SWIM and GAIN activity, Appendix Table A.3 indicates that during the five-year follow-up period, 66 percent of the AFDC-FG experimentals and 42 percent of the AFDC-FG controls within the casefile sample participated in activities thought to enhance individuals' employment prospects, resulting in incremental participation totalling 24 percentage points. Among the AFDC-Us, 75 percent of the experimentals and 33 percent of the controls within the casefile sample participated in such activities during the follow-up period, resulting in incremental participation totalling 42 percentage points.

TABLE A.2

SWIM-GAIN CASEFILE STUDY

GAIN ACTIVITY ESTIMATES BY YEAR OF FOLLOW-UP PERIOD, ASSISTANCE CATEGORY, AND RESEARCH GROUP

Activity Measure	AFDC-FG Experi-mentals	AFDC-FG Con-trols	AFDC-FG Differ-ence	AFDC-U Experi-mentals	AFDC-U Con-trols	AFDC-U Differ-ence
Attended GAIN Orientation (%)						
Within one year of follow-up	0.0	0.0	0.0	0.0	0.0	0.0
Within two years of follow-up	5.4	3.3	2.1	3.1	1.5	1.6
Within three years of follow-up	21.6	18.7	2.9	12.5	12.1	0.4
Within four years of follow-up	27.7	30.1	-2.4	14.1	27.3	-13.2 *
Within five years of follow-up	30.4	30.9	-0.5	14.1	27.3	-13.2 *
Participated in any GAIN activity (%) (job search, PREP, education, training, or self-initiated education or training)						
Within one year of follow-up	0.0	0.0	0.0	0.0	0.0	0.0
Within two years of follow-up	1.4	1.6	-0.2	0.0	0.0	0.0
Within three years of follow-up	10.1	10.6	-0.5	4.7	9.1	-4.4
Within four years of follow-up	15.5	18.7	-3.2	7.8	16.7	-8.9
Within five years of follow-up	17.6	19.5	-1.9	7.8	19.7	-11.9 **
Deregistered due to sanction (%)						
Within one year of follow-up	0.0	0.0	0.0	0.0	0.0	0.0
Within two years of follow-up	0.0	0.0	0.0	0.0	0.0	0.0
Within three years of follow-up	0.7	0.0	0.7	0.0	0.0	0.0
Within four years of follow-up	1.4	1.6	-0.2	1.6	0.0	1.6
Within five years of follow-up	1.4	4.1	-2.7	1.6	0.0	1.6
Sample size (AFDC-FG total = 271) (AFDC-U total = 130)	148	123		64	66	

SOURCE: MDRC calculations from GAIN casefile records maintained by the County of San Diego Department of Social Services.

NOTES: The sample for this table consists of a subsample of individuals who registered with SWIM between July 1985 and June 1986.

Activity measures are calculated as a percent of the total number of persons in the indicated assistance category and research group. Follow-up begins with the date of initial SWIM registration and ends five years later.

Participation is defined as attending a GAIN job search activity, PREP (GAIN work experience), GAIN-referred education or training, or GAIN-approved client-initiated education or training for at least one day.

A two-tailed t-test was applied to differences between experimental and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

TABLE A.3

SWIM-GAIN CASEFILE STUDY

PERCENT OF SWIM IMPACT SAMPLE MEMBERS IN VARIOUS SWIM AND/OR GAIN ACTIVITIES OR STATUSES DURING THE FIVE YEARS FOLLOWING INITIAL SWIM REGISTRATION, BY TYPE OF ACTIVITY/STATUS, WHETHER IT OCCURRED DURING SWIM OR GAIN, ASSISTANCE CATEGORY, AND RESEARCH GROUP

	AFDC-FG			AFDC-U		
Activity Measure	Experimentals	Controls	Difference	Experimentals	Controls	Difference
Participated in any activity (job search, work experience, education, training, or self-initiated education or training)						
Participated during SWIM (%)	62.8	30.1	32.7 ***	73.4	21.2	52.2 ***
Participated during GAIN (%)	17.6	19.5	-1.9	7.8	19.7	-11.9 **
Participated during SWIM or GAIN (%)	66.2	42.3	23.9 ***	75.0	33.3	41.7 ***
Participated during both SWIM and GAIN (%)	14.2	7.3	6.9 *	6.3	7.6	-1.3
Sanctioned						
Sanctioned during SWIM (%)	9.5	0	9.5 ***	9.4	0	9.4 **
Deregistered due to sanction during GAIN (%)	1.4	4.1	-2.7	1.6	0	1.6
Sanctioned during SWIM or deregistered due to sanction during GAIN (%)	10.1	4.1	6.0 *	10.9	0.0	10.9 ***
Sanctioned or deregistered due to sanction during both SWIM and GAIN (%)	0.7	0	0.7	0.0	0.0	0.0
Sample size (AFDC-FG total = 271) (AFDC-U total = 130)	148	123		64	66	

(continued)

Table A.3 (continued).

SOURCE: MDRC calculations from the County of San Diego Department of Social Services SWIM Automated Tracking System and EWEP attendance logs; the San Diego Community College District Student Information System; the San Diego County JTPA Management Information System; and GAIN casefile records maintained by the County of San Diego Department of Social Services.

NOTES: The sample for this table consists of a subsample of individuals who registered with SWIM between July 1985 and June 1986.

"Participated during SWIM" is defined as attending a job search activity for at least one day, attending EWEP for at least one hour, enrolling in a community college program for at least one day, or attending a JTPA-funded activity for at least one day between the date of initial SWIM registration and June 30, 1988.

"Participated during GAIN" is defined as attending a GAIN job search activity, PREP (GAIN work experience), GAIN-referred education or training, or GAIN-approved client-initiated education or training for at least one day between the date of initial GAIN orientation and five years from the date of initial SWIM registration.

"Sanctioned during SWIM" is defined as having one's AFDC grant reduced due to noncompliance with SWIM program requirements.

"Deregistered due to sanction during GAIN" is defined as having been deregistered from GAIN due to a request from GAIN staff to Income Maintenance staff asking that the client's AFDC grant be reduced due to noncompliance with GAIN program requirements.

A two-tailed t-test was applied to differences between experimental and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

APPENDIX B

SUPPLEMENTAL TABLES TO CHAPTER 4

TABLE B.1

SWIM

ALL AFDC-FG: IMPACTS ON EMPLOYMENT, EARNINGS, AFDC RECEIPT, AND AFDC PAYMENTS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Ever employed (%)				
Quarters 2-21	74.6	67.5	7.1 ***	10.5%
Quarters 2-5	51.7	40.4	11.3 ***	28.0%
Quarters 6-9	49.4	40.2	9.2 ***	23.0%
Quarters 10-13	47.1	40.4	6.7 ***	16.7%
Quarters 14-17	43.4	42.0	1.4	3.4%
Quarters 18-21	43.3	41.7	1.6	3.9%
Average quarterly employment rate (%)				
Quarters 2-21	33.9	29.0	4.8 ***	16.7%
Quarters 2-5	33.0	25.7	7.3 ***	28.4%
Quarters 6-9	35.2	27.9	7.2 ***	25.9%
Quarters 10-13	34.4	28.2	6.2 ***	22.0%
Quarters 14-17	33.5	31.3	2.2	7.0%
Quarters 18-21	33.3	32.0	1.3	4.0%
Ever employed (%)				
Quarter of random assignment	27.9	25.1	2.7 **	10.9%
Quarter 2	30.8	24.7	6.1 ***	24.9%
Quarter 3	33.0	25.5	7.5 ***	29.3%
Quarter 4	33.7	25.8	7.8 ***	30.3%
Quarter 5	34.7	26.9	7.8 ***	28.9%
Quarter 6	34.9	26.7	8.3 ***	31.0%
Quarter 7	35.6	27.5	8.2 ***	29.7%
Quarter 8	35.2	28.3	6.9 ***	24.2%
Quarter 9	34.9	29.2	5.6 ***	19.2%
Quarter 10	35.2	28.6	6.6 ***	23.1%
Quarter 11	34.6	27.8	6.8 ***	24.3%
Quarter 12	33.5	27.6	5.8 ***	21.1%
Quarter 13	34.1	28.6	5.5 ***	19.4%
Quarter 14	33.0	30.2	2.8 *	9.3%
Quarter 15	33.3	31.0	2.3	7.5%
Quarter 16	33.9	32.1	1.7	5.4%
Quarter 17	33.8	32.0	1.9	5.9%
Quarter 18	33.3	31.1	2.2	7.2%
Quarter 19	33.9	32.2	1.7	5.4%
Quarter 20	33.6	32.7	0.9	2.7%
Quarter 21	32.3	32.0	0.3	0.9%

(continued)

Table B.1 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Average total earnings ($)				
Quarters 2-21, annualized	3222	2807	415 **	14.8%
Quarters 2-5	2029	1678	352 ***	21.0%
Quarters 6-9	2892	2248	644 ***	28.6%
Quarters 10-13	3287	2732	555 ***	20.3%
Quarters 14-17	3775	3397	378	11.1%
Quarters 18-21	4126	3978	148	3.7%
Average earnings per quarter employed ($)				
Quarters 2-21	*2378*	*2417*	*-39 (a)*	*-1.6%*
Quarters 2-5	*1535*	*1629*	*-94 (a)*	*-5.8%*
Quarters 6-9	*2056*	*2012*	*44 (a)*	*2.2%*
Quarters 10-13	*2391*	*2424*	*-33 (a)*	*-1.4%*
Quarters 14-17	*2816*	*2712*	*105 (a)*	*3.9%*
Quarters 18-21	*3100*	*3108*	*-9 (a)*	*-0.3%*
Average total earnings ($)				
Quarter of random assignment	275	271	4	1.4%
Quarter 2	365	339	27	7.9%
Quarter 3	486	401	85 **	21.2%
Quarter 4	568	456	112 ***	24.6%
Quarter 5	610	482	128 ***	26.5%
Quarter 6	678	485	193 ***	39.8%
Quarter 7	710	547	164 ***	29.9%
Quarter 8	737	596	141 ***	23.7%
Quarter 9	766	621	145 ***	23.4%
Quarter 10	800	646	154 ***	23.8%
Quarter 11	818	657	161 ***	24.4%
Quarter 12	821	698	123 **	17.5%
Quarter 13	849	731	118 **	16.2%
Quarter 14	909	774	135 **	17.4%
Quarter 15	938	836	101	12.1%
Quarter 16	958	886	71	8.1%
Quarter 17	971	901	70	7.8%
Quarter 18	1005	947	58	6.2%
Quarter 19	1053	977	77	7.9%
Quarter 20	1039	1026	14	1.3%
Quarter 21	1028	1028	-1	-0.1%

(continued)

Table B.1 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Ever received any AFDC payments (%)				
Quarters 2-21	92.9	93.2	-0.3	-0.3%
Quarters 2-22	93.0	93.3	-0.3	-0.3%
Quarters 2-5	91.3	92.1	-0.8	-0.9%
Quarters 6-9	64.4	71.5	-7.1 ***	-9.9%
Quarters 10-13	53.1	58.9	-5.9 ***	-9.9%
Quarters 14-17	44.9	48.2	-3.3 **	-6.9%
Quarters 18-21	38.9	41.5	-2.6	-6.2%
Average number of months receiving AFDC payments				
Quarters 2-21	28.46	31.31	-2.85 ***	-9.1%
Quarters 2-22	29.36	32.25	-2.89 ***	-9.0%
Quarters 2-5	8.60	9.13	-0.53 ***	-5.9%
Quarters 6-9	6.34	7.23	-0.89 ***	-12.3%
Quarters 10-13	5.22	5.95	-0.73 ***	-12.2%
Quarters 14-17	4.41	4.87	-0.45 **	-9.3%
Quarters 18-21	3.89	4.13	-0.25	-6.0%
Ever received any AFDC payments (%)				
Quarter of random assignment	91.1	91.5	-0.4	-0.4%
Quarter 2	89.7	89.9	-0.2	-0.2%
Quarter 3	79.0	81.6	-2.6 **	-3.2%
Quarter 4	70.6	76.2	-5.6 ***	-7.3%
Quarter 5	66.0	72.5	-6.5 ***	-9.0%
Quarter 6	60.9	68.3	-7.4 ***	-10.8%
Quarter 7	57.3	64.8	-7.5 ***	-11.6%
Quarter 8	53.7	60.7	-7.0 ***	-11.5%
Quarter 9	51.3	58.8	-7.4 ***	-12.7%
Quarter 10	48.3	55.2	-6.9 ***	-12.5%
Quarter 11	46.6	53.4	-6.7 ***	-12.6%
Quarter 12	45.2	50.6	-5.5 ***	-10.8%
Quarter 13	44.0	48.3	-4.3 ***	-8.9%
Quarter 14	41.3	45.4	-4.1 **	-9.1%
Quarter 15	39.1	42.7	-3.6 **	-8.4%
Quarter 16	38.0	41.6	-3.5 **	-8.5%
Quarter 17	36.9	40.3	-3.5 **	-8.6%
Quarter 18	36.0	38.4	-2.4	-6.2%
Quarter 19	34.1	36.9	-2.8 *	-7.5%
Quarter 20	33.5	35.7	-2.2	-6.2%
Quarter 21	32.6	33.5	-0.9	-2.7%
Quarter 22	31.5	32.5	-1.0	-3.1%

(continued)

Table B.1 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference		Percent Difference
Average total AFDC payments received ($)					
Quarters 2-21, annualized	3145	3528	-383	***	-10.9%
Quarters 2-22, annualized	3096	3468	-373	***	-10.7%
Quarters 2-5	4419	4838	-419	***	-8.7%
Quarters 6-9	3407	3968	-560	***	-14.1%
Quarters 10-13	2952	3435	-483	***	-14.1%
Quarters 14-17	2621	2905	-284	**	-9.8%
Quarters 18-21	2327	2496	-169		-6.8%
Average AFDC payment per month received ($)					
Quarters 2-21	*553*	*563*	*-11*	*(a)*	*-1.9%*
Quarters 2-22	*554*	*565*	*-11*	*(a)*	*-1.9%*
Quarters 2-5	*514*	*530*	*-16*	*(a)*	*-3.0%*
Quarters 6-9	*538*	*549*	*-11*	*(a)*	*-2.0%*
Quarters 10-13	*565*	*577*	*-12*	*(a)*	*-2.1%*
Quarters 14-17	*594*	*597*	*-3*	*(a)*	*-0.6%*
Quarters 18-21	*599*	*604*	*-5*	*(a)*	*-0.8%*
Average AFDC payments received ($)					
Quarter of random assignment	1192	1196	-4		-0.3%
Quarter 2	1284	1335	-51	***	-3.8%
Quarter 3	1118	1227	-108	***	-8.8%
Quarter 4	1030	1162	-132	***	-11.4%
Quarter 5	986	1114	-128	***	-11.5%
Quarter 6	921	1067	-147	***	-13.7%
Quarter 7	866	1013	-147	***	-14.5%
Quarter 8	825	964	-139	***	-14.4%
Quarter 9	795	923	-128	***	-13.9%
Quarter 10	766	897	-131	***	-14.6%
Quarter 11	738	875	-136	***	-15.6%
Quarter 12	729	849	-120	***	-14.1%
Quarter 13	718	815	-96	***	-11.8%
Quarter 14	679	766	-86	***	-11.3%
Quarter 15	651	728	-77	**	-10.6%
Quarter 16	652	711	-58	*	-8.2%
Quarter 17	638	700	-62	**	-8.9%
Quarter 18	619	669	-50		-7.4%
Quarter 19	588	639	-50	*	-7.9%
Quarter 20	567	609	-42		-6.9%
Quarter 21	553	580	-27		-4.7%
Quarter 22	527	567	-40		-7.1%
Sample size (total = 3210)	1604	1606			

SOURCES AND NOTES: See Table 4.1.

(a) Not an experimental comparison; statistical tests not performed.

TABLE B.2

SWIM

ALL AFDC-FG: IMPACTS ON COMBINED EMPLOYMENT AND AFDC RECEIPT STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference
Not employed, received AFDC (%)			
Average quarterly rate			
Quarters 2-21	37.3	43.4	-6.1 ***
Quarters 2-5	54.2	62.7	-8.6 ***
Quarters 6-9	40.8	50.6	-9.8 ***
Quarters 10-13	34.5	42.2	-7.8 ***
Quarters 14-17	30.2	33.0	-2.8 *
Quarters 18-21	26.9	28.7	-1.8
Quarterly rate			
Quarter of random assignment	67.0	69.4	-2.4 *
Quarter 2	63.5	69.7	-6.2 ***
Quarter 3	56.4	63.8	-7.4 ***
Quarter 4	50.3	60.4	-10.0 ***
Quarter 5	46.5	57.0	-10.5 ***
Quarter 6	43.7	54.9	-11.2 ***
Quarter 7	41.0	52.2	-11.3 ***
Quarter 8	39.9	48.3	-8.5 ***
Quarter 9	38.5	46.8	-8.3 ***
Quarter 10	35.6	45.0	-9.3 ***
Quarter 11	34.8	43.5	-8.7 ***
Quarter 12	34.5	41.5	-7.0 ***
Quarter 13	32.8	38.9	-6.1 ***
Quarter 14	31.1	35.3	-4.2 ***
Quarter 15	30.3	33.6	-3.2 **
Quarter 16	30.2	31.7	-1.4
Quarter 17	29.0	31.3	-2.3
Quarter 18	28.8	31.0	-2.2
Quarter 19	26.2	29.5	-3.3 **
Quarter 20	26.4	27.7	-1.2
Quarter 21	26.2	26.6	-0.4

(continued)

Table B.2 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference	
Employed, received AFDC (%)				
Average quarterly rate				
Quarters 2-21	12.9	11.3	1.6	***
Quarters 2-5	22.1	17.3	4.8	***
Quarters 6-9	15.0	12.6	2.5	***
Quarters 10-13	11.6	9.6	1.9	**
Quarters 14-17	8.6	9.5	-0.9	
Quarters 18-21	7.1	7.4	-0.3	
Quarterly rate				
Quarter of random assignment	24.1	22.1	2.0	
Quarter 2	26.2	20.2	6.0	***
Quarter 3	22.6	17.7	4.8	***
Quarter 4	20.3	15.8	4.5	***
Quarter 5	19.5	15.5	4.0	***
Quarter 6	17.2	13.4	3.8	***
Quarter 7	16.3	12.6	3.7	***
Quarter 8	13.8	12.3	1.5	
Quarter 9	12.9	12.0	0.9	
Quarter 10	12.7	10.2	2.4	**
Quarter 11	11.8	9.9	1.9	*
Quarter 12	10.6	9.1	1.6	
Quarter 13	11.2	9.4	1.8	*
Quarter 14	10.1	10.1	0.1	
Quarter 15	8.8	9.1	-0.3	
Quarter 16	7.8	9.9	-2.1	**
Quarter 17	7.9	9.0	-1.2	
Quarter 18	7.2	7.4	-0.2	
Quarter 19	7.9	7.4	0.5	
Quarter 20	7.1	8.0	-1.0	
Quarter 21	6.4	6.9	-0.5	

(continued)

Table B.2 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference
Employed, did not receive AFDC (%)			
Average quarterly rate			
Quarters 2-21	21.0	17.7	3.2 ***
Quarters 2-5	10.9	8.4	2.5 ***
Quarters 6-9	20.1	15.4	4.8 ***
Quarters 10-13	22.8	18.5	4.3 ***
Quarters 14-17	24.9	21.8	3.1 **
Quarters 18-21	26.1	24.6	1.6
Quarterly rate			
Quarter of random assignment	3.8	3.0	0.8
Quarter 2	4.6	4.5	0.1
Quarter 3	10.5	7.8	2.7 ***
Quarter 4	13.4	10.0	3.4 ***
Quarter 5	15.2	11.5	3.8 ***
Quarter 6	17.7	13.3	4.5 ***
Quarter 7	19.4	14.9	4.4 ***
Quarter 8	21.4	16.0	5.4 ***
Quarter 9	22.0	17.3	4.8 ***
Quarter 10	22.6	18.4	4.2 ***
Quarter 11	22.8	18.0	4.8 ***
Quarter 12	22.9	18.6	4.3 ***
Quarter 13	23.0	19.2	3.8 ***
Quarter 14	22.9	20.1	2.7 *
Quarter 15	24.6	21.9	2.6 *
Quarter 16	26.1	22.2	3.8 ***
Quarter 17	26.0	22.9	3.0 **
Quarter 18	26.1	23.7	2.4
Quarter 19	26.0	24.8	1.2
Quarter 20	26.5	24.7	1.9
Quarter 21	25.9	25.1	0.8

(continued)

Table B.2 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference
Not employed, did not receive AFDC (%)			
Average quarterly rate			
Quarters 2-21	28.8	27.5	1.3
Quarters 2-5	12.8	11.5	1.2
Quarters 6-9	24.1	21.5	2.6 **
Quarters 10-13	31.2	29.6	1.6
Quarters 14-17	36.3	35.7	0.6
Quarters 18-21	39.8	39.3	0.5
Quarterly rate			
Quarter of random assignment	5.1	5.4	-0.4
Quarter 2	5.7	5.6	0.1
Quarter 3	10.6	10.6	-0.0
Quarter 4	16.0	13.8	2.2 *
Quarter 5	18.8	16.0	2.7 **
Quarter 6	21.4	18.4	3.0 **
Quarter 7	23.4	20.3	3.1 **
Quarter 8	24.9	23.3	1.6
Quarter 9	26.7	24.0	2.7 *
Quarter 10	29.1	26.4	2.7 *
Quarter 11	30.6	28.7	1.9
Quarter 12	32.0	30.8	1.2
Quarter 13	33.0	32.5	0.5
Quarter 14	35.9	34.5	1.4
Quarter 15	36.4	35.4	0.9
Quarter 16	35.9	36.2	-0.3
Quarter 17	37.1	36.7	0.4
Quarter 18	37.8	37.9	-0.1
Quarter 19	39.9	38.3	1.5
Quarter 20	40.0	39.6	0.3
Quarter 21	41.5	41.4	0.1
Sample size (total = 3210)	1604	1606	

SOURCES AND NOTES: See Table 4.1.

TABLE B.3

SWIM

AFDC-FG SUBGROUPS: IMPACTS ON EARNINGS IN QUARTERS 6-9 AND 18-21

Subgroup	Percent of Sample	Average Total Earnings, Quarters 6-9 ($) Experi-mentals	Controls	Difference	(b)	Average Total Earnings, Quarters 18-21 ($) Experi-mentals	Controls	Difference	(b)
FULL SAMPLE	100.0	2892	2248	644	***	4126	3978	148	
IMPLEMENTATION									
SWIM office									
Service Center	49.9	2613	2203	410	*	3922	3925	-3	
San Diego West	50.1	3172	2295	877	***	4330	4031	299	
Random assignment cohort									
July-September 1985	29.6	2586	2237	349		4152	3869	283	
October-December 1985	25.2	3034	2370	664	**	3607	3996	-389	
January-March 1986	24.3	2996	2275	721	**	4276	4086	190	
April-June 1986	21.0	3026	2086	940	***	4520	3990	530	
BARRIERS TO EMPLOYMENT									
Prior year earnings									
$3,000 or more	17.7	5923	4813	1110	***	7548	6827	720	
$1-$2,999	21.6	4612	4159	453		3927	4614	-687	
None	60.7	1952	1389	564	***	3206	2918	288	
High school diploma				xx					
Yes	56.1	3756	2813	944	***	5189	5230	-42	
No	43.9	1784	1525	259		2766	2375	391	
Number of own children (a)				x					
One	45.7	3357	2315	1041	***	4305	4122	183	
Two	29.7	2957	2414	543	*	4290	4086	205	
More than two	20.2	2080	1875	205		3639	3490	149	

(continued)

Table B.3 (continued).

Subgroup	Percent of Sample	Average Total Earnings, Quarters 6-9 ($) Experi-mentals	Controls	Difference	(b)	Average Total Earnings, Quarters 18-21 ($) Experi-mentals	Controls	Difference	(b)
AFDC HISTORY AND STATUS									
Had own AFDC case									
Never	11.2	3789	2860	929	*	5349	4092	1258	
Two years or less	20.2	4161	3455	706	*	5288	4944	344	
More than two years	68.6	2371	1791	579	***	3576	3670	-94	
Welfare status				x					
Applicant	39.2	3269	3010	259		4466	4383	83	
Recipient	60.8	2655	1764	891	***	3908	3718	190	
Level of disadvantage				xx					
First-time applicants	8.6	4219	3096	1123	**	5531	4043	1488	
Returning applicants	30.6	3001	2986	15		4163	4481	-318	
Less disadvantaged recipients	42.4	3251	2170	1081	***	4780	4534	246	
More disadvantaged recipients	18.4	1259	816	444		1877	1846	31	
ETHNICITY									
White, non-Hispanic	27.3	3474	2752	721	**	4431	4607	-176	
Black, non-Hispanic	42.4	2603	2143	460	*	3900	3956	-56	
Hispanic	25.4	2639	1901	738	**	3889	3219	670	
Asian and other	4.9	3491	2194	1297	*	5894	4513	1381	

(continued)

Table B.3 (continued).

Subgroup	Percent of Sample	Average Total Earnings, Quarters 6-9 ($) Experi-mentals	Controls	Difference	(b)	Average Total Earnings, Quarters 18-21 ($) Experi-mentals	Controls	Difference	(b)
SPECIAL SUBGROUPS									
Hispanic: English Speaking				(d)				(d)	
Yes	17.4	2968	2255	713	*	4509	3864	645	
No	8.0	1925	1132	793		2544	1822	723	
Activity status at random assignment (c)				(d)				(d)	
In school	14.9	3791	2835	957	**	6141	6170	-28	
In any activity	15.5	3717	2631	1086	**	5869	6127	-258	
Previously in program activity	37.1	2827	2229	599	**	4263	4240	23	
Employed more than 20 hours per week	5.4	4318	3854	464		5974	5404	570	
Sample size (total= 3210)		1604	1606			1604	1606		

SOURCES AND NOTES: See Table 4.1.

(a) Results for a fraction of the sample reporting zero "own children" are excluded from the table.

(b) An F-test was applied to subgroup differences in "difference" estimates. Statistical significance levels are indicated as: x = 10 percent; xx = 5 percent; xxx = 1 percent.

(c) These categories are not mutually exclusive and do not include sample members with no activity status at random assignment.

(d) F-test not applied.

TABLE B.4

SWIM

AFDC-FG SUBGROUPS: IMPACTS ON AFDC PAYMENTS IN QUARTERS 6-9 AND 18-21

Subgroup	Percent of Sample	Average Total AFDC Payments, Quarters 6-9 ($) Experimentals	Controls	Difference	(b)	Average Total AFDC Payments, Quarters 18-21 ($) Experimentals	Controls	Difference	(b)
FULL SAMPLE	100.0	3407	3968	-560	***	2327	2496	-169	
IMPLEMENTATION									
SWIM office									
Service Center	49.9	3848	4274	-426	***	2564	2770	-207	
San Diego West	50.1	2967	3662	-694	***	2093	2224	-131	
Random assignment cohort									
July-September 1985	29.6	3390	3977	-587	***	2292	2512	-221	
October-December 1985	25.2	3137	3695	-558	***	2223	2260	-37	
January-March 1986	24.3	3705	4147	-442	**	2590	2566	24	
April-June 1986	21.0	3409	4079	-670	***	2199	2682	-484	*
BARRIERS TO EMPLOYMENT									
Prior year earnings									
$3,000 or more	17.7	2479	2730	-250		1824	1848	-24	
$1-$2,999	21.6	3220	3690	-470	**	2217	2378	-161	
None	60.7	3749	4429	-680	***	2516	2728	-212	
High school diploma									
Yes	56.1	2997	3594	-597	***	2003	2191	-189	
No	43.9	3933	4446	-513	***	2743	2887	-143	
Number of own children (a)									
One	45.7	2507	3247	-740	***	1694	2057	-363	**
Two	29.7	3548	4019	-471	**	2586	2449	137	
More than two	20.2	5226	5657	-431	*	3427	3662	-235	

(continued)

Table B.4 (continued).

Subgroup	Percent of Sample	Average Total AFDC Payments, Quarters 6-9 ($)				Average Total AFDC Payments, Quarters 18-21 ($)			
		Experi-mentals	Controls	Difference	(b)	Experi-mentals	Controls	Difference	(b)
AFDC HISTORY AND STATUS									
Had own AFDC case									
Never	11.2	2544	2957	-413		1530	1666	-136	
Two years or less	20.2	2215	2755	-540	**	1555	1710	-155	
More than two years	68.6	3896	4492	-596	***	2685	2863	-178	
Welfare status									
Applicant	39.2	2356	2828	-472	***	1631	1897	-266	
Recipient	60.8	4083	4700	-618	***	2777	2883	-106	
Level of disadvantage									
First-time applicants	8.6	2020	2525	-505		1141	1533	-392	
Returning applicants	30.6	2447	2917	-470	***	1767	2002	-235	
Less disadvantaged recipients	42.4	3815	4462	-647	***	2589	2709	-121	
More disadvantaged recipients	18.4	4685	5266	-581	**	3200	3296	-96	
ETHNICITY									
White, non-Hispanic	27.3	2393	3236	-843	***	1845	1846	-1	
Black, non-Hispanic	42.4	3769	4234	-465	***	2527	2690	-163	
Hispanic	25.4	3897	4291	-393	**	2514	2985	-471	**
Asian and other	4.9	3515	4024	-509		2392	1950	442	

(continued)

Table B.4 (continued).

Subgroup	Percent of Sample	Average Total AFDC Payments, Quarters 6-9 ($) Experi-mentals	Controls	Difference	(b)	Average Total AFDC Payments, Quarters 18-21 ($) Experi-mentals	Controls	Difference	(b)
SPECIAL SUBGROUPS									
Hispanic: English speaking				(d)				(d)	
Yes	17.4	3916	4031	-115		2508	2903	-396	
No	8.0	3858	4854	-996	***	2529	3163	-633	
Activity status at random assignment (c)				(d)				(d)	
In school	14.9	3663	4136	-473	*	2354	2453	-98	
In any activity	15.5	3765	4245	-479	*	2349	2488	-139	
Previously in program activity	37.1	3738	4155	-417	**	2517	2673	-156	
Employed more than 20 hours per week	5.4	2710	3349	-639		1981	2701	-720	
Sample size (total= 3210)		1604	1606			1604	1606		

SOURCES AND NOTES: See Table 4.1.

(a) Results for a fraction of the sample reporting zero "own children" are excluded from the table.

(b) An F-test was applied to subgroup differences in "difference" estimates. Statistical significance levels are indicated as: x = 10 percent; xx = 5 percent; xxx = 1 percent

(c) These categories are not mutually exclusive and do not include sample members with no activity status at random assignment.

(d) F-test not applied.

APPENDIX C

SUPPLEMENTAL TABLES TO CHAPTER 5

TABLE C.1

SWIM

ALL AFDC-U: IMPACTS ON EMPLOYMENT, EARNINGS, AFDC RECEIPT, AND AFDC PAYMENTS

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Ever employed (%)				
Quarters 2-21	79.3	75.4	4.0 *	5.2%
Quarters 2-5	58.3	49.4	8.9 ***	18.0%
Quarters 6-9	57.8	51.6	6.2 **	12.0%
Quarters 10-13	53.3	48.8	4.5 *	9.3%
Quarters 14-17	52.5	49.0	3.5	7.2%
Quarters 18-21	48.8	45.0	3.9	8.6%
Average quarterly employment rate (%)				
Quarters 2-21	39.4	35.7	3.7 **	10.4%
Quarters 2-5	37.5	32.0	5.6 ***	17.5%
Quarters 6-9	41.8	36.9	4.8 **	13.1%
Quarters 10-13	40.1	36.3	3.8 *	10.4%
Quarters 14-17	40.2	37.6	2.6	7.0%
Quarters 18-21	37.3	35.6	1.8	4.9%
Ever employed (%)				
Quarter of random assignment	38.0	35.7	2.2	6.2%
Quarter 2	35.9	29.3	6.6 ***	22.4%
Quarter 3	37.5	31.9	5.7 **	17.8%
Quarter 4	38.7	32.9	5.8 **	17.8%
Quarter 5	38.0	33.7	4.2 *	12.6%
Quarter 6	39.7	36.5	3.2	8.6%
Quarter 7	42.0	37.2	4.8 *	12.8%
Quarter 8	41.7	37.0	4.7 *	12.8%
Quarter 9	43.8	37.1	6.6 ***	17.9%
Quarter 10	42.6	36.6	6.0 **	16.5%
Quarter 11	39.4	35.5	3.9	11.0%
Quarter 12	39.2	37.0	2.2	5.9%
Quarter 13	39.0	36.0	2.9	8.1%
Quarter 14	41.0	37.8	3.2	8.5%
Quarter 15	39.6	37.2	2.4	6.4%
Quarter 16	40.5	38.4	2.2	5.7%
Quarter 17	39.6	36.9	2.7	7.4%
Quarter 18	39.3	38.0	1.2	3.2%
Quarter 19	38.2	36.2	2.0	5.5%
Quarter 20	36.4	34.7	1.7	4.8%
Quarter 21	35.4	33.2	2.1	6.4%

(continued)

Table C.1 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Average total earnings ($)				
Quarters 2-21, annualized	4576	4364	212	4.9%
Quarters 2-5	3303	2815	487 *	17.3%
Quarters 6-9	4308	3831	478	12.5%
Quarters 10-13	4797	4448	350	7.9%
Quarters 14-17	5211	5214	-4	-0.1%
Quarters 18-21	5259	5510	-251	-4.6%
Average earnings per quarter employed ($)				
Quarters 2-21	*2905*	*3059*	*-154 (a)*	*-5.0%*
Quarters 2-5	*2200*	*2202*	*-3 (a)*	*-0.1%*
Quarters 6-9	*2578*	*2592*	*-14 (a)*	*-0.5%*
Quarters 10-13	*2994*	*3063*	*-69 (a)*	*-2.3%*
Quarters 14-17	*3242*	*3471*	*-229 (a)*	*-6.6%*
Quarters 18-21	*3524*	*3874*	*-350 (a)*	*-9.0%*
Average total earnings ($)				
Quarter of random assignment	560	540	19	3.6%
Quarter 2	659	557	103	18.4%
Quarter 3	838	688	150 *	21.9%
Quarter 4	883	770	114	14.8%
Quarter 5	922	802	121	15.1%
Quarter 6	938	890	48	5.4%
Quarter 7	1074	931	143	15.4%
Quarter 8	1131	981	150	15.2%
Quarter 9	1166	1029	137	13.3%
Quarter 10	1240	1048	192 *	18.3%
Quarter 11	1162	1059	103	9.7%
Quarter 12	1167	1147	21	1.8%
Quarter 13	1228	1194	34	2.9%
Quarter 14	1304	1306	-2	-0.2%
Quarter 15	1282	1298	-15	-1.2%
Quarter 16	1307	1306	1	0.1%
Quarter 17	1318	1305	13	1.0%
Quarter 18	1297	1442	-145	-10.1%
Quarter 19	1341	1413	-73	-5.1%
Quarter 20	1368	1316	52	3.9%
Quarter 21	1253	1339	-85	-6.4%

(continued)

Table C.1 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Ever received any AFDC payments (%)				
Quarters 2-21	90.0	90.2	-0.2	-0.2%
Quarters 2-22	90.0	90.3	-0.4	-0.4%
Quarters 2-5	86.7	86.8	-0.1	-0.1%
Quarters 6-9	60.6	65.4	-4.8 *	-7.3%
Quarters 10-13	53.0	54.8	-1.8	-3.3%
Quarters 14-17	46.8	47.7	-0.9	-1.9%
Quarters 18-21	44.8	44.0	0.9	1.9%
Average number of months receiving AFDC payments				
Quarters 2-21	27.79	29.14	-1.34	-4.6%
Quarters 2-22	28.93	30.27	-1.34	-4.4%
Quarters 2-5	7.59	7.95	-0.35	-4.4%
Quarters 6-9	5.86	6.31	-0.44	-7.0%
Quarters 10-13	5.12	5.46	-0.34	-6.2%
Quarters 14-17	4.68	4.88	-0.20	-4.2%
Quarters 18-21	4.54	4.54	-0.00	-0.1%
Ever received any AFDC payments (%)				
Quarter of random assignment	86.0	84.4	1.6	1.9%
Quarter 2	83.7	84.0	-0.3	-0.3%
Quarter 3	67.6	71.3	-3.7	-5.2%
Quarter 4	64.9	67.7	-2.9	-4.3%
Quarter 5	60.4	62.8	-2.5	-3.9%
Quarter 6	54.6	59.2	-4.6 *	-7.8%
Quarter 7	52.5	57.6	-5.0 **	-8.8%
Quarter 8	51.2	54.2	-3.0	-5.5%
Quarter 9	49.5	50.8	-1.3	-2.6%
Quarter 10	47.4	50.3	-2.9	-5.8%
Quarter 11	45.2	49.8	-4.6 *	-9.2%
Quarter 12	45.3	46.5	-1.2	-2.5%
Quarter 13	42.7	44.6	-1.9	-4.3%
Quarter 14	41.5	43.5	-2.1	-4.8%
Quarter 15	40.8	43.3	-2.6	-5.9%
Quarter 16	41.2	41.9	-0.7	-1.6%
Quarter 17	40.9	41.6	-0.7	-1.8%
Quarter 18	40.0	40.3	-0.3	-0.7%
Quarter 19	38.6	39.2	-0.7	-1.7%
Quarter 20	39.5	38.8	0.7	1.7%
Quarter 21	39.7	39.2	0.5	1.3%
Quarter 22	40.0	39.7	0.3	0.7%

(continued)

Table C.1 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference	Percent Difference
Average total AFDC payments received ($)				
Quarters 2-21, annualized	3819	4211	-392 **	-9.3%
Quarters 2-22, annualized	3796	4174	-379 **	-9.1%
Quarters 2-5	4888	5303	-415 **	-7.8%
Quarters 6-9	3896	4455	-558 ***	-12.5%
Quarters 10-13	3558	4036	-479 **	-11.9%
Quarters 14-17	3406	3730	-324	-8.7%
Quarters 18-21	3345	3530	-185	-5.2%
Average AFDC payment per month received ($)				
Quarters 2-21	*687*	*723*	*-36 (a)*	*-4.9%*
Quarters 2-22	*689*	*724*	*-35 (a)*	*-4.9%*
Quarters 2-5	*644*	*667*	*-24 (a)*	*-3.6%*
Quarters 6-9	*664*	*706*	*-42 (a)*	*-5.9%*
Quarters 10-13	*695*	*739*	*-44 (a)*	*-6.0%*
Quarters 14-17	*728*	*764*	*-36 (a)*	*-4.7%*
Quarters 18-21	*737*	*777*	*-40 (a)*	*-5.1%*
Average AFDC payments received ($)				
Quarter of random assignment	1266	1275	-9	-0.7%
Quarter 2	1424	1469	-45	-3.1%
Quarter 3	1193	1323	-130 ***	-9.8%
Quarter 4	1168	1283	-114 **	-8.9%
Quarter 5	1102	1228	-126 **	-10.3%
Quarter 6	1019	1173	-153 ***	-13.1%
Quarter 7	1007	1134	-128 **	-11.3%
Quarter 8	950	1088	-138 **	-12.7%
Quarter 9	920	1060	-140 **	-13.2%
Quarter 10	906	1035	-129 **	-12.5%
Quarter 11	894	1024	-131 **	-12.7%
Quarter 12	891	995	-105 *	-10.5%
Quarter 13	867	981	-114 **	-11.6%
Quarter 14	853	957	-103 *	-10.8%
Quarter 15	856	943	-86	-9.1%
Quarter 16	850	928	-78	-8.4%
Quarter 17	846	903	-57	-6.3%
Quarter 18	842	902	-60	-6.7%
Quarter 19	825	890	-65	-7.3%
Quarter 20	837	880	-43	-4.9%
Quarter 21	841	857	-17	-1.9%
Quarter 22	836	862	-26	-3.1%
Sample size (total = 1340)	686	654		

SOURCES AND NOTES: See Table 4.1.

(a) Not an experimental comparison; statistical tests not performed.

TABLE C.2

SWIM

ALL AFDC-U: IMPACTS ON COMBINED
EMPLOYMENT AND AFDC RECEIPT STATUS

Outcome and Follow-Up Period	Experimentals	Controls	Difference
Not employed, received AFDC (%)			
Average quarterly rate			
Quarters 2-21	33.4	37.5	-4.1 **
Quarters 2-5	47.6	53.8	-6.2 ***
Quarters 6-9	34.0	39.0	-5.0 **
Quarters 10-13	30.1	35.2	-5.1 **
Quarters 14-17	27.7	30.7	-3.0
Quarters 18-21	27.5	28.6	-1.1
Quarterly rate			
Quarter of random assignment	56.3	57.6	-1.3
Quarter 2	56.3	63.0	-6.7 ***
Quarter 3	47.6	54.4	-6.8 ***
Quarter 4	44.4	51.1	-6.7 ***
Quarter 5	42.3	46.8	-4.5 *
Quarter 6	37.9	41.0	-3.1
Quarter 7	34.6	40.5	-5.8 **
Quarter 8	33.5	37.8	-4.3 *
Quarter 9	30.2	36.9	-6.7 ***
Quarter 10	30.4	36.9	-6.4 ***
Quarter 11	30.9	36.7	-5.8 **
Quarter 12	29.6	33.9	-4.3 *
Quarter 13	29.4	33.1	-3.7
Quarter 14	27.5	31.4	-3.8
Quarter 15	27.6	31.3	-3.7
Quarter 16	27.7	29.8	-2.1
Quarter 17	27.9	30.3	-2.4
Quarter 18	26.2	28.8	-2.6
Quarter 19	26.4	28.1	-1.6
Quarter 20	28.4	28.5	-0.1
Quarter 21	29.2	29.2	-0.0

(continued)

Table C.2 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference
Employed, received AFDC (%)			
Average quarterly rate			
Quarters 2-21	16.0	13.9	2.1 *
Quarters 2-5	21.5	17.6	3.8 **
Quarters 6-9	17.9	16.4	1.5
Quarters 10-13	15.1	12.7	2.4
Quarters 14-17	13.4	11.9	1.5
Quarters 18-21	11.9	10.8	1.1
Quarterly rate			
Quarter of random assignment	29.7	26.8	2.8
Quarter 2	27.4	21.0	6.4 ***
Quarter 3	20.0	16.9	3.1
Quarter 4	20.4	16.6	3.8 *
Quarter 5	18.1	16.1	2.0
Quarter 6	16.7	18.2	-1.5
Quarter 7	17.9	17.1	0.8
Quarter 8	17.7	16.4	1.3
Quarter 9	19.3	14.0	5.4 ***
Quarter 10	16.9	13.4	3.5 *
Quarter 11	14.3	13.1	1.2
Quarter 12	15.7	12.6	3.2 *
Quarter 13	13.3	11.5	1.8
Quarter 14	13.9	12.2	1.7
Quarter 15	13.2	12.0	1.2
Quarter 16	13.5	12.1	1.4
Quarter 17	13.0	11.3	1.6
Quarter 18	13.8	11.5	2.3
Quarter 19	12.1	11.1	1.0
Quarter 20	11.1	10.3	0.8
Quarter 21	10.5	10.0	0.5

(continued)

Table C.2 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference
Employed, did not receive AFDC (%)			
Average quarterly rate			
Quarters 2-21	23.4	21.8	1.6
Quarters 2-5	16.1	14.3	1.7
Quarters 6-9	23.9	20.5	3.3 *
Quarters 10-13	25.0	23.6	1.3
Quarters 14-17	26.8	25.6	1.1
Quarters 18-21	25.4	24.8	0.6
Quarterly rate			
Quarter of random assignment	8.3	8.9	-0.6
Quarter 2	8.5	8.4	0.1
Quarter 3	17.5	15.0	2.6
Quarter 4	18.3	16.3	2.0
Quarter 5	19.9	17.7	2.2
Quarter 6	22.9	18.3	4.6 **
Quarter 7	24.0	20.0	4.0 *
Quarter 8	24.0	20.5	3.5
Quarter 9	24.4	23.2	1.3
Quarter 10	25.7	23.2	2.5
Quarter 11	25.1	22.4	2.7
Quarter 12	23.5	24.4	-1.0
Quarter 13	25.6	24.5	1.1
Quarter 14	27.1	25.6	1.5
Quarter 15	26.4	25.2	1.2
Quarter 16	27.0	26.3	0.8
Quarter 17	26.7	25.6	1.1
Quarter 18	25.5	26.5	-1.1
Quarter 19	26.1	25.1	1.0
Quarter 20	25.3	24.4	0.9
Quarter 21	24.8	23.2	1.6

(continued)

Table C.2 (continued).

Outcome and Follow-Up Period	Experimentals	Controls	Difference
Not employed, did not receive AFDC (%)			
Average quarterly rate			
Quarters 2-21	27.2	26.9	0.4
Quarters 2-5	14.8	14.2	0.6
Quarters 6-9	24.2	24.0	0.2
Quarters 10-13	29.9	28.5	1.3
Quarters 14-17	32.1	31.7	0.4
Quarters 18-21	35.1	35.8	-0.7
Quarterly rate			
Quarter of random assignment	5.7	6.7	-1.0
Quarter 2	7.8	7.7	0.2
Quarter 3	14.9	13.7	1.2
Quarter 4	16.8	16.0	0.8
Quarter 5	19.7	19.5	0.2
Quarter 6	22.5	22.5	-0.0
Quarter 7	23.4	22.4	1.1
Quarter 8	24.8	25.2	-0.5
Quarter 9	26.1	26.0	0.0
Quarter 10	26.9	26.5	0.4
Quarter 11	29.7	27.8	1.9
Quarter 12	31.2	29.1	2.1
Quarter 13	31.7	30.8	0.8
Quarter 14	31.5	30.9	0.6
Quarter 15	32.8	31.5	1.4
Quarter 16	31.8	31.8	-0.1
Quarter 17	32.4	32.8	-0.4
Quarter 18	34.5	33.2	1.4
Quarter 19	35.4	35.7	-0.3
Quarter 20	35.2	36.8	-1.5
Quarter 21	35.5	37.6	-2.1
Sample size (total = 1340)	686	654	

SOURCES AND NOTES: See Table 4.1.

TABLE C.3

SWIM

AFDC-U SUBGROUPS: IMPACTS ON EARNINGS IN QUARTERS 6-9 AND 18-21

Subgroup	Percent of Sample	Average Total Earnings, Quarters 6-9 ($) Experimentals	Controls	Difference	(b)	Average Total Earnings, Quarters18-21 ($) Experimentals	Controls	Difference	(b)
FULL SAMPLE	100.0	4308	3831	478		5259	5510	-251	
IMPLEMENTATION									
SWIM office									
Service center	50.7	4586	4585	0		5585	5799	-214	
San Diego West	49.3	4023	3051	973	**	4924	5213	-290	
Random assignment cohort									
July-September 1985	29.4	4402	4047	355		5818	5745	73	
October-December 1985	24.6	4650	3083	1567	**	5341	5148	193	
January-March 1986	23.1	4022	3434	588		4900	4632	268	
April-June 1986	22.9	4080	4734	-655		4773	6476	-1702	*
BARRIERS TO EMPLOYMENT									
Prior year earnings									
$3,000 or more	32.1	7028	6510	518		8566	8560	7	
$1-$2,999	24.5	3506	3410	96		4192	5022	-830	
None	43.4	2744	2099	644		3440	3509	-68	
High school diploma									
Yes	47.0	5001	4200	801		6912	6573	339	
No	53.0	3700	3506	194		3803	4572	-769	
Number of own children (a)								x	
One	26.5	5057	3627	1430	**	6515	5212	1303	
Two	31.3	4221	4457	-236		5881	5963	-81	
More than two	39.9	3934	3448	485		3906	5519	-1613	**

(continued)

Table C.3 (continued).

Subgroup	Percent of Sample	Average Total Earnings, Quarters 6-9 ($) Experi-mentals	Controls	Difference	(b)	Average Total Earnings, Quarters18-21 ($) Experi-mentals	Controls	Difference	(b)
AFDC HISTORY AND STATUS									
Had own AFDC case									
Never	33.7	5395	4877	518		6393	6572	-179	
Two years or less	34.6	4284	4126	158		5387	5825	-438	
More than two years	31.8	3183	2409	774		3919	4044	-126	
Welfare status									
Applicant	59.5	5253	4712	541		6295	6030	265	
Recipient	40.5	2926	2540	386		3764	4768	-1004	
Level of disadvantage									
First-time applicants	30.6	5565	5111	454		6622	6617	5	
Returning applicants	28.9	4916	4307	609		5952	5440	513	
Less disadvantaged recipients	29.3	3121	2934	187		4273	5740	-1468	
More disadvantaged recipients	11.2	2458	1518	940		2504	2270	234	
ETHNICITY									
White, non-Hispanic	25.6	4131	4137	-6		5552	5763	-211	
Black, non-Hispanic	20.8	4695	3417	1278	*	5620	5138	482	
Hispanic	40.2	4752	4256	496		5682	5349	334	
Asian and other	13.4	2664	2566	98		2828	6004	-3176	**

(continued)

Table C.3 (continued).

Subgroup	Percent of Sample	Average Total Earnings, Quarters 6-9 ($) Experi-mentals	Controls	Difference	(b)	Average Total Earnings, Quarters18-21 ($) Experi-mentals	Controls	Difference	(b)
SPECIAL SUBGROUPS									
Hispanic: English Speaking				(d)				(d)	
Yes	26.9	5817	4434	1383	**	7223	5741	1482	
No	13.3	2410	3803	-1393		2241	4498	-2257	*
Activity status at random assignment (c)				(d)				(d)	
In school	8.9	3930	3352	577		5390	6871	-1481	
In any activity	10.3	4246	3177	1068		5955	6169	-214	
Previously in program activity	29.6	3974	3263	711		4743	5125	-382	
Employed more than 20 hours per week	3.2	3890	6935	-3045		4216	6461	-2245	
Sample size (total= 1340)		686	654			686	654		

(a) Results for a fraction of the sample reporting zero "own children" are excluded from the table.

(b) An F-test was applied to subgroup differences in "difference" estimates. Statistical significance levels are indicated as: x = 10 percent; xx = 5 percent; xxx = 1 percent.

(c) These categories are not mutually exclusive and do not include sample members with no activity status at random assignment.

(d) F-test not applied.

TABLE C.4

SWIM

AFDC-U SUBGROUPS: IMPACTS ON AFDC PAYMENTS IN QUARTERS 6-9 AND 18-21

Subgroup	Percent of Sample	Average Total AFDC Payments, Quarters 6-9 ($)				Average Total AFDC Payments, Quarters 18-21 ($)			
		Experi-mentals	Controls	Difference	(b)	Experi-mentals	Controls	Difference	(b)
FULL SAMPLE	100.0	3896	4455	-558	***	3345	3530	-185	
IMPLEMENTATION									
SWIM office									
Service Center	50.7	3942	4490	-548	**	3472	3735	-263	
San Diego West	49.3	3849	4419	-569	**	3215	3319	-104	
Random assignment cohort									
July-September 1985	29.4	3596	4227	-631	*	3168	3285	-118	
October-December 1985	24.6	3746	4879	-1133	***	3053	3931	-878	*
January-March 1986	23.1	4045	4303	-258		3807	3628	179	
April-June 1986	22.9	4316	4447	-130		3443	3318	125	
BARRIERS TO EMPLOYMENT									
Prior year earnings									
$3,00 or more	32.1	3040	3223	-183		3040	2993	47	
$1-$2,999	24.5	3764	4736	-973	**	3470	3500	-29	
None	43.4	4602	5206	-603	**	3502	3938	-436	
High school diploma								x	
Yes	47.0	3492	3757	-265		3066	2828	238	
No	53.0	4260	5076	-816	***	3601	4157	-556	*
Number of own children (a)									
One	26.5	2582	2935	-352		2540	2324	216	
Two	31.3	3853	4069	-216		2744	3211	-467	
More than two	39.9	4841	5909	-1068	***	4402	4656	-254	

(continued)

Table C.4 (continued).

Subgroup	Percent of Sample	Average Total AFDC Payments, Quarters 6-9 ($) Experi-mentals	Controls	Difference	(b)	Average Total AFDC Payments, Quarters 18-21 ($) Experi-mentals	Controls	Difference	(b)
AFDC HISTORY AND STATUS									
Had own AFDC case									
Never	33.7	2851	3031	-180		2741	2383	359	
Two years or less	34.6	3685	4334	-650	*	3018	3474	-455	
More than two years	31.8	5223	6083	-860	**	4325	4798	-473	
Welfare status									
Applicant	59.5	3015	3419	-403		2834	2811	23	
Recipient	40.5	5197	5981	-784	**	4107	4595	-488	
Level of disadvantage				x					
First-time applicants	30.6	2687	2784	-97		2512	2132	380	
Returning applicants	28.9	3341	4069	-728	**	3155	3499	-343	
Less disadvantaged recipients	29.3	5074	5479	-405		3999	4261	-263	
More disadvantaged recipients	11.2	5444	7269	-1825	***	4333	5465	-1132	*
ETHNICITY				x				xx	
White, non-Hispanic	25.6	2820	2835	-16		1909	2251	-341	
Black, non-Hispanic	20.8	4086	4830	-745	*	4203	3649	554	
Hispanic	40.2	4181	4565	-384		3633	3571	62	
Asian and other	13.4	4837	6600	-1763	***	3900	5566	-1666	***

(continued)

Table C.4 (continued).

Subgroup	Percent of Sample	Average Total AFDC Payments, Quarters 6-9 ($)			Average Total AFDC Payments, Quarters 18-21 ($)		
		Experimentals	Controls	Difference (b)	Experimentals	Controls	Difference (b)
SPECIAL SUBGROUPS							
Hispanic: English speaking				(d)			(d)
Yes	26.9	3955	4462	-507	3581	3688	-107
No	13.3	4635	4813	-178	3723	3393	330
Activity status at random assignment (c)				(d)			(d)
In school	8.9	4173	5413	-1240 *	2763	4587	-1824 **
In any activity	10.3	4366	5114	-748	2846	4148	-1302 *
Previously in program activity	29.6	4734	5407	-672 *	3746	4485	-739 *
Employed more than 20 hours per week	3.2	1998	3177	-1179	1536	2072	-535
Sample size (total= 1340)		686	654		686	654	

SOURCES AND NOTES: See Table 4.1.

(a) Results for a fraction of the sample reporting zero "own children" are excluded from the table.

(b) An F-test was applied to subgroup differences in "difference" estimates. Statistical significance levels are indicated as: x = 10 percent; xx = 5 percent; xxx = 1 percent.

(c) These categories are not mutually exclusive and do not include sample members with no activity status at random assignment.

(d) F-test not applied.

APPENDIX D

SUBGROUP BENEFIT-COST ANALYSIS

Detailed results of the subgroup benefit-cost analysis are shown in Tables D.1 through D.12. The analysis was performed for the four-way breakdown introduced in the main text: first-time applicants, returning applicants, less disadvantaged recipients, and more disadvantaged recipients. The analysis was also performed for applicants as a group and recipients as a group. Each table presents estimates for one of these subgroups. The format of all tables is identical and also matches the format of the benefit-cost tables in the main text. Methodology was the same as for the full sample, except that the computations were made separately for each of the subsamples. Thus, the present value of earnings gains was calculated six times for the six AFDC-FG subgroups and six times for the AFDC-U subgroups. Net costs were calculated separately, too. These costs are derived from separate estimates of incremental activity in the various SWIM program components and in education and training provided elsewhere in the community.[1]

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[1] Several of the valuations of activities were not computed separately for each subgroup but were, instead, supplied from full-sample estimates.

TABLE D.1

SWIM

AFDC-FG APPLICANTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	803	0	-803	0
Fringe benefits	96	0	-96	0
Output produced by participants				
EWEP	0	0	123	123
Employment	0	0	899	899
Tax payments		(b)		
Payroll taxes	-53	118	53	0
Income and sales taxes	-18	18	18	0
Transfer programs				
AFDC payments	-1547	1547	1547	0
Payments from other programs	-272	272	272	0
Transfer administrative costs	0	47	47	47
		(c)		
SWIM operating costs	0	-476	-476	-476
Support service and allowances	57	-57	-57	0
Use of community education and training programs	0	-138	-138	-138
Estimated GAIN costs	0	-22	-22	-22
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	-935	1310	1369	434

(continued)

Table D.1 (continued).

SOURCE: See Table 5.1.

NOTES: Results are expressed in 1986 dollars. Differences are regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members. The AFDC-FG sample of applicants includes 646 experimentals and 611 controls, and the AFDC-U sample of applicants includes 398 experimentals and 399 controls. Because of rounding, details may not sum to totals.

(a) The net present value is the sum of all gains and losses within each perspective.
(b) The upper boxed-in estimates, totalling $2,002, represent the five-year per experimental benefits from the government budget perspective.
(c) The lower boxed-in estimates, totalling $692, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $2.89.

TABLE D.2

SWIM

AFDC-FG FIRST-TIME APPLICANTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	5026	0	-5026	0
Fringe benefits	603	0	-603	0
Output produced by participants				
EWEP	0	0	150	150
Employment	0	0	5629	5629
Tax payments		(b)		
Payroll taxes	-340	750	340	0
Income and sales taxes	-526	526	526	0
Transfer programs				
AFDC payments	-1828	1828	1828	0
Payments from other programs	-99	99	99	0
Transfer administrative costs	0	22	22	22
SWIM operating costs	0	(c) -483	-483	-483
Support service and allowances	54	-54	-54	0
Use of community education and training programs	0	-97	-97	-97
Estimated GAIN costs	0	-22	-22	-22
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	2890	2569	2308	5199

(continued)

Table D.2 (continued).

SOURCE: See Table 5.1.

NOTES: Results are expressed in 1986 dollars. Differences are regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members. The AFDC-FG sample of first-time applicants includes 142 experimentals and 134 controls, and the AFDC-U sample of first-time applicants includes 218 experimentals and 192 controls. Because of rounding, details may not sum to totals.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling $3,225, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $657, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $4.91.

TABLE D.3

SWIM

AFDC-FG RETURNING APPLICANTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	-390	0	390	0
Fringe benefits	-47	0	47	0
Output produced by participants				
EWEP	0	0	116	116
Employment	0	0	-437	-437
Tax payments		(b)		
Payroll taxes	27	-60	-27	0
Income and sales taxes	127	-127	-127	0
Transfer programs				
AFDC payments	-1493	1493	1493	0
Payments from other programs	-324	324	324	0
Transfer administrative costs	0	54	54	54
SWIM operating costs	0	(c) -474	-474	-474
Support service and allowances	57	-57	-57	0
Use of community education and training programs	0	-149	-149	-149
Estimated GAIN costs	0	-22	-22	-22
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	-2043	982	1131	-912

(continued)

Table D.3 (continued).

SOURCE: See Table 5.1.

NOTES: Results are expressed in 1986 dollars. Differences are regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members. The AFDC-FG sample of returning applicants includes 504 experimentals and 477 controls, and the AFDC-U sample of returning applicants includes 180 experimentals and 207 controls. Because of rounding, details may not sum to totals.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling $1,684, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $703, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $2.40.

TABLE D.4

SWIM

AFDC-FG RECIPIENTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	2442	0	-2442	0
Fringe benefits	293	0	-293	0
Output produced by participants				
EWEP	0	0	219	219
Employment	0	0	2735	2735
Tax payments		(b)		
Payroll taxes	-178	390	178	0
Income and sales taxes	67	-67	-67	0
Transfer programs				
AFDC payments	-1740	1740	1740	0
Payments from other programs	-157	157	157	0
Transfer administrative costs	0	31	31	31
SWIM operating costs	0	(c) -637	-637	-637
Support service and allowances	83	-83	-83	0
Use of community education and training programs	0	-325	-325	-325
Estimated GAIN costs	0	-30	-30	-30
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	810	1175	1182	1992

(continued)

Table D.4 (continued).

SOURCE: See Table 5.1.

NOTES: Results are expressed in 1986 dollars. Differences are regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members. The AFDC-FG sample of recipients includes 958 experimentals and 995 controls, and the AFDC-U sample of recipients includes 288 experimentals and 255 controls. Because of rounding, details may not sum to totals.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling $2,251, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $1075, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $2.09.

TABLE D.5

SWIM

AFDC-FG LESS DISADVANTAGED RECIPIENTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	2917	0	-2917	0
Fringe benefits	350	0	-350	0
Output produced by participants				
EWEP	0	0	176	176
Employment	0	0	3268	3268
Tax payments		(b)		
Payroll taxes	-212	465	212	0
Income and sales taxes	24	-24	-24	0
Transfer programs				
AFDC payments	-1782	1782	1782	0
Payments from other programs	-111	111	111	0
Transfer administrative costs	0	29	29	29
SWIM operating costs	0	(c) -578	-578	-578
Support service and allowances	71	-71	-71	0
Use of community education and training programs	0	-250	-250	-250
Estimated GAIN costs	0	-30	-30	-30
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	1257	1434	1357	2614

(continued)

Table D.5 (continued).

SOURCE: See Table 5.1.

NOTES: Results are expressed in 1986 dollars. Differences are regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members. The AFDC-FG sample of less disadvantaged recipients includes 684 experimentals and 678 controls, and the AFDC-U sample of less disadvantaged recipients includes 214 experimentals and 179 controls. Because of rounding, details may not sum to totals.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling $2,363, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $929, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $2.54.

TABLE D.6

SWIM

AFDC-FG MORE DISADVANTAGED RECIPIENTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	1309	0	-1309	0
Fringe benefits	157	0	-157	0
Output produced by participants				
EWEP	0	0	326	326
Employment	0	0	1466	1466
Tax payments		(b)		
Payroll taxes	-96	211	96	0
Income and sales taxes	175	-175	-175	0
Transfer programs				
AFDC payments	-1756	1756	1756	0
Payments from other programs	-274	274	274	0
Transfer administrative costs	0	39	39	39
SWIM operating costs	0	(c) -785	-785	-785
Support service and allowances	101	-101	-101	0
Use of community education and training programs	0	-471	-471	-471
Estimated GAIN costs	0	-30	-30	-30
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	-385	717	928	544

(continued)

Table D.6 (continued).

SOURCE: See Table 5.1.

NOTES: Results are expressed in 1986 dollars. Differences are regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members. The AFDC-FG sample of more disadvantaged recipients includes 274 experimentals and 317 controls, and the AFDC-U sample of more disadvantaged recipients includes 74 experimentals and 76 controls. Because of rounding, details may not sum to totals.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling $2,104, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $1388, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $1.52.

TABLE D.7

SWIM

AFDC-U APPLICANTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	1767	0	-1767	0
Fringe benefits	212	0	-212	0
Output produced by participants				
EWEP	0	0	221	221
Employment	0	0	1979	1979
Tax payments		(b)		
Payroll taxes	-128	281	128	0
Income and sales taxes	-258	258	258	0
Transfer programs				
AFDC payments	-763	763	763	0
Payments from other programs	-146	146	146	0
Transfer administrative costs	0	18	18	18
SWIM operating costs	0	(c) -539	-539	-539
Support service and allowances	45	-45	-45	0
Use of community education and training programs	0	-79	-79	-79
Estimated GAIN costs	0	-14	-14	-14
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	728	790	858	1586

(continued)

Table D.7 (continued).

SOURCES AND NOTES: See Table D.1.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling $1,467, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $677, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $2.17.

TABLE D.8

SWIM

AFDC-U FIRST-TIME APPLICANTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	1034	0	-1034	0
Fringe benefits	124	0	-124	0
Output produced by participants				
EWEP	0	0	181	181
Employment	0	0	1158	1158
Tax payments		(b)		
Payroll taxes	-76	167	76	0
Income and sales taxes	-173	173	173	0
Transfer programs				
AFDC payments	502	-502	-502	0
Payments from other programs	31	-31	-31	0
Transfer administrative costs	0	-22	-22	-22
		(c)		
SWIM operating costs	0	-498	-498	-498
Support service and allowances	39	-39	-39	0
Use of community education and training programs	0	-21	-21	-21
Estimated GAIN costs	0	-14	-14	-14
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	1481	-787	-697	784

(continued)

Table D.8 (continued).

SOURCES AND NOTES: See Table D.2.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling -$215, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $572, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets lost another $.38.

TABLE D.9

SWIM

AFDC-U RETURNING APPLICANTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	2407	0	-2407	0
Fringe benefits	289	0	-289	0
Output produced by participants				
EWEP	0	0	269	269
Employment	0	0	2696	2696
Tax payments		(b)		
Payroll taxes	-173	381	173	0
Income and sales taxes	-328	328	328	0
Transfer programs				
AFDC payments	-2074	2074	2074	0
Payments from other programs	-335	335	335	0
Transfer administrative costs	0	60	60	60
		(c)		
SWIM operating costs	0	-588	-588	-588
Support service and allowances	52	-52	-52	0
Use of community education and training programs	0	-131	-131	-131
Estimated GAIN costs	0	-14	-14	-14
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	-163	2393	2455	2292

(continued)

Table D.9 (continued).

SOURCES AND NOTES: See Table D.3.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling $3,178, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $785, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $4.05.

TABLE D.10

SWIM

AFDC-U RECIPIENTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	189	0	-189	0
Fringe benefits	23	0	-23	0
Output produced by participants				
EWEP	0	0	344	344
Employment	0	0	211	211
Tax payments		(b)		
Payroll taxes	-12	27	12	0
Income and sales taxes	203	-203	-203	0
Transfer programs				
AFDC payments	-3067	3067	3067	0
Payments from other programs	91	-91	-91	0
Transfer administrative costs	0	45	45	45
SWIM operating costs	0	(c) -610	-610	-610
Support service and allowances	55	-55	-55	0
Use of community education and training programs	0	-364	-364	-364
Estimated GAIN costs	0	-27	-27	-27
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	-2519	1788	2118	-401

(continued)

Table D.10 (continued).

SOURCES AND NOTES: See Table D.4.

(a) The net present value is the sum of all gains and losses within each perspective.
(b) The upper boxed-in estimates, totalling $2,845, represent the five-year per experimental benefits from the government budget perspective.
(c) The lower boxed-in estimates, totalling $1,056, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $2.69.

TABLE D.11

SWIM

AFDC-U LESS DISADVANTAGED RECIPIENTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	-825	0	825	0
Fringe benefits	-99	0	99	0
Output produced by participants				
EWEP	0	0	325	325
Employment	0	0	-924	-924
Tax payments		(b)		
Payroll taxes	63	-137	-63	0
Income and sales taxes	188	-188	-188	0
Transfer programs				
AFDC payments	-2004	2004	2004	0
Payments from other programs	359	-359	-359	0
Transfer administrative costs	0	14	14	14
		(c)		
SWIM operating costs	0	-596	-596	-596
Support service and allowances	57	-57	-57	0
Use of community education and training programs	0	-437	-437	-437
Estimated GAIN costs	0	-27	-27	-27
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	-2261	217	616	-1645

(continued)

Table D.11 (continued).

SOURCES AND NOTES: See Table D.5.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling $1,334, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $1117, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $1.19.

TABLE D.12

SWIM

AFDC-U MORE DISADVANTAGED RECIPIENTS: ESTIMATED BENEFITS AND COSTS PER EXPERIMENTAL OVER FIVE YEARS BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
Component of Analysis	Welfare Sample	Budget	Taxpayer	Society
Earnings	2917	0	-2917	0
Fringe benefits	350	0	-350	0
Output produced by participants				
EWEP	0	0	396	396
Employment	0	0	3268	3268
Tax payments		(b)		
Payroll taxes	-214	468	214	0
Income and sales taxes	242	-242	-242	0
Transfer programs				
AFDC payments	-6039	6039	6039	0
Payments from other programs	-633	633	633	0
Transfer administrative costs	0	132	132	132
SWIM operating costs	0	(c) -649	-649	-649
Support service and allowances	49	-49	-49	0
Use of community education and training programs	0	-171	-171	-171
Estimated GAIN costs	0	-27	-27	-27
Preference for work over welfare	+	0	+	+
Foregone personal and family activities	-	0	0	-
Value of education not reflected in earnings	+	0	+	+
Net present value (a)	-3327	6133	6275	2948

(continued)

Table D.12 (continued).

SOURCES AND NOTES: See Table D.6.

(a) The net present value is the sum of all gains and losses within each perspective.

(b) The upper boxed-in estimates, totalling $7,030, represent the five-year per experimental benefits from the government budget perspective.

(c) The lower boxed-in estimates, totalling $896, represent the five-year per experimental costs from the government budget perspective. The upper box benefits divided by the lower box costs indicate that, for every dollar spent on SWIM, government budgets saved $7.84.

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